EMPIRICAL DATA ON THE SCORING OF TRUE-FALSE TESTS

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In a recently compiled bibliography of fifty-six references to new type examinations, twenty related to or discussed the true-false type of examination.[1] Eight of these references contained experimental evidence concerning the reliability and validity of true-false tests utilizing the traditional right minus wrong scoring formula; eleven advocated or severely criticized, on theoretical grounds, the use of true-false tests and the right minus wrong scoring method in connection therewith; only one reference presented experimental evidence regarding the comparative reliability of true-false tests when scored right minus wrong or when scored number right.[2]

In all of this work, psychologists have proceeded to utilize the true-false test on the basis of the following assumptions, explicit or implied:

a. Guessing enters into true-false tests to a marked extent.

b. The effect of guessing should be and is counteracted by a right minus wrong scoring formula.

c. The right minus wrong scoring method is more reliable than merely scoring by the number right or by the number wrong.

PREVIOUS WORK ON BEST SCORING METHODS FOR TRUE-FALSE TESTS

Ruch's discussion of these assumptions is penetrating, leading him to secure experimental data which throw serious

[1] Donald G. Paterson, The Preparation and Use of New Type Examinations, Appendix B, Annotated Bibliography, Published by the World Book Company, New York, 1925.

[2] G. M. Ruch, The Improvement of Written Examinations, ix, pp. 1-193, Published by Scott, Foresman Company, New York, 1924.

doubt on the truth of these assumptions.[3] His data were derived from duplicate forms of two-alternative response tests in social science and history and also from duplicate forms of the three two-alternative response tests in the Terman Group Test for Mental Ability. A summary of his results follow:

TEST	RELIABILITY FOR DUPLICATE FORMS OF 50 ITEMS EACH	
	Scored number right	Scored right minus wrong
Ruch, 2-response (135 cases)	+0.74±0.027	+0.68±0.031
Ruch, true-false (133 cases)	+0.56±0.040	+0.41±0.049
Terman, Test 3, Word Meaning, 2-response (43 cases)	+0.67±0.056	+0.56±0.071
Terman, Test 6, Sentence Meaning, Yes-No (43 cases)	+0.53±0.073	+0.47±0.080
Terman, Test 8, Mixed Sentences, True-False (43 cases)	+0.68±0.056	+0.56±0.071

In each and every instance the number right method of scoring results in higher reliability coefficients than the right minus wrong method.

The writers are aware of only one other reference making experimental comparisons of these two methods of scoring, i.e., analyses made in determining the best methods of scoring the army intelligence tests.[4] In discussing "examination a" the army report states:

> The tests were scored in various ways in order to determine the best method empirically. The principle adopted was that of finding by trial the method of scoring which would give the highest correlation with Binet or with total scores. All the tests were scored both for "number right" and "right minus wrong." In addition, Test 4 was also scored for "right − 2 wrong"; and Tests 5 and 7 for "3 right − wrong." It has been supposed that it would be found necessary to penalize, more or less extensively, for errors. However, it was found that, in general, penalizing was of little or no advantage except in those tests where it was

[3] G. M. Ruch, op. cit., pp. 114–120.

[4] Memoirs of the National Academy of Sciences, Vol. XV, 1921.

necessary to counterbalance the factor of chance, as in Tests 3 and 6. Test 4, for example, gave significantly higher correlations with Binet and with total score when scored for number right than when scored for "right − wrong" or "right − 2 wrong." Only in Tests 3, 6, 7, and 10 did the data justify subtraction from the score in case of error. In Tests 3 and 6 the subject has an even chance of making a correct response by guessing, and this is offset by scoring the test "right − wrong." In Tests 7 and 10 the advantage gained by penalizing for errors was small.[5]

One is unable to verify the soundness of the above conclusions because no data, in the form of number of cases, means, sigmas, reliability coefficients, or probable errors are given.

In a section devoted to methods of scoring "examination a," the same problem is presented with more details.[6] A study made at Camp Lee for a group of random cases revealed the following for the 2 response tests:

	CORRELATIONS WITH TOTAL EXAMINATION SCORES	
	Number right	Right minus wrong
Test 3, True-false	+0.66±0.028	+0.72±0.024
Test 6, Same-opposites	+0.76±0.034	+0.82±0.026

Here, we find a slight advantage for the right minus wrong method. However, the difference in favor of the right minus wrong method is not statistically significant, i.e., the difference in neither case is equal to 4 or even 3 times the probable error of the difference. These results, it is to be noted, are not in terms of reliability but in terms of validity according to current usage. The importance of this asserted advantage is lessened by the next sentence, "Similar data collected at Camp Dix indicated a slight advantage for the right minus wrong method for test 3 and no advantage for this method in the case of test 6." The above constitutes the sole evidence for the adoption of right minus wrong methods in scoring these two tests which became tests 5 and 4 in the Alpha examination.

[5] Memoirs, op. cit., p. 305.

[6] Memoirs, op. cit., p. 339.

RELIABILITY COEFFICIENTS FOR DIFFERENT SCORING METHODS

The above presentation of available evidence indicates the need for additional data in order that this question of the best method of scoring two-alternative response tests may be more definitely established. The writers, therefore, present the following data to be considered in connection with

TABLE 1

Reliability coefficients for 100 item true-false test in psychology of advertising according to the method of 50 odd items vs. 50 even items

SCORING METHOD	CLASS IN 1924 (61 CASES)	CLASS IN 1925 (50 CASES)	BOTH CLASSES COMBINED (111 CASES)
Number right	+0.57±0.060	+0.73±0.045	+0.63±0.037
Right minus wrong	+0.49±0.065	+0.60±0.061	+0.54±0.045
Number wrong	+0.46±0.069	+0.57±0.063	+0.51±0.048

TABLE 2

Statistical constants for groups in table 1

	CLASS IN 1924		CLASS IN 1925		BOTH CLASSES COMBINED	
	M.	S.D.	M.	S.D.	M.	S.D.
Number right, odds	41.9	3.6	40.8	3.7	41.4	3.7
Number right, evens	43.1	3.5	42.1	3.1	42.6	3.4
Right-wrong, odds	35.2	5.9	33.9	6.0	34.6	6.0
Right-wrong, evens	36.9	5.9	36.1	5.5	36.6	5.8
Number wrong, odds	7.3	2.7	7.4	3.0	7.3	2.9
Number wrong, evens	6.6	2.6	6.2	2.8	6.4	2.7

the preceding results and to serve as a stimulus for additional contributions of a like nature in various school subjects. The results here reported were obtained from a 100 item true-false test (unlimited time allowed) covering twelve weeks' work in the Psychology of Advertising, involving 61 juniors and seniors in 1924 and 50 juniors and seniors in 1925. The students were cautioned against guessing and informed that a right minus wrong scoring formula would be used in grading

their papers. Reliability coefficients were derived by correlating 50 odd items vs. 50 even items.[7] This method, while not the best method of measuring reliability or consistency, is not open to the charge of spuriously magnifying consistency which frequently results when odd items are correlated with even items in *time limit tests*. The reliability coefficients are summarized in table 1.

The means and standard deviations of the distributions for the above groups are presented in table 2. Inspection of table 1 reveals the fact that for both classes combined and for each class separately the number right scoring method yields the highest reliability coefficients, the right minus wrong method yields the next highest and the number wrong method yields the lowest reliability coefficients. The differences, even in the case of the combined classes, are not statistically significant yet they duplicate Ruch's results. Hence, the assumption that the right minus wrong method is more reliable than the number right method of scoring true-false tests is seriously questioned by the facts. Being unable to explain the differences in the reliability coefficients for the 1924 class and the 1925 class, the writers have included the means and sigmas in table 2. In no case are the differences statistically significant, the inference being that 50 item true-false tests with groups of fifty or sixty students will yield fluctuating reliability coefficients from group to group. A difference of as much as 16 points may occur and still be accounted for on the basis of chance variations in the repeated experimental conditions.

VALIDITY COEFFICIENTS FOR DIFFERENT SCORING METHODS

In determining the validity of the different scoring methods, average grades in all examinations in the course constituted

[7] All correlations reported in this paper were computed by the Pearson Product Moment formula, each and every step being double checked. The reliability coefficients in each instance were computed from class intervals of 1 each, thus avoiding any possible errors due to grouping scores in class intervals.

the criterion, the results of the true-false tests themselves being omitted. In 1924 the criterion was a combination of essay and objective type examinations. In 1925 objective type examinations only were used, consisting of completion and recognition items. In combining average scholarship for the two classes the mean of each was made equal to 50, tenths of sigma being counted as 1 each. A scholarship score of +1 sigma thus becomes 60 and a scholarship score of −1 sigma

TABLE 3

Correlation between each scoring method for 100 item true-false test and average scholarship

SCORING METHOD	CLASS OF 1924 (61 CASES)	CLASS OF 1925 (50 CASES)	BOTH CLASSES COMBINED (111 CASES)
Right minus wrong	+0.39±0.073	+0.52±0.070	+0.44±0.050
Number right	+0.38±0.074	+0.49±0.072	+0.39±0.054
Number wrong	−0.35±0.076	−0.52±0.070	−0.44±0.051

TABLE 4

Statistical constants for variables in table 3

	CLASS OF 1924 (61 CASES)		CLASS OF 1925 (50 CASES)		BOTH CLASSES COMBINED (111 CASES)	
	M.	S.D.	M.	S.D.	M.	S.D.
Average scholarship (criterion)	50.4	9.7	53.1	10.1	51.6	9.9
Right minus wrong	71.4	10.1	69.8	10.1	70.7	10.1
Number right	84.7	6.0	82.4	6.4	83.6	6.3
Number wrong	13.4	4.7	13.0	5.2	13.3	4.9

thus becomes 40. The assumptions involved in this procedure are: (1) the two classes are of equal ability on the average, (2) the variability in each class is the same, and (3) the distribution of scholarship in both classes conforms to the normal curve. There is reason to believe that these assumptions are sufficiently correct to warrant the combination of scores in the manner indicated.

Table 3 presents the validity coefficients and table 4 presents the means and standard deviations of the variables involved.

Reference to table 3 shows slight differences between the validity coefficients for the various scoring methods. Such differences as do exist are not statistically significant. The data, although indicating the possibility of a slightly higher validity for the right minus wrong method, fail to substantiate the oft repeated claims for the necessity and desirability of the right minus wrong scoring formula. Indeed, here we have evidence that merely scoring the number wrong is as valid as making the correction for chance although no one has ever seriously proposed such a method of scoring true-false tests.

Inspection of table 4 shows that the correction for chance (right minus wrong method) lowers the mean scores in the 100 item test to a marked extent, at the same time greatly increasing the standard deviations. This greater variability would be a decided gain were it accompanied by increased reliability, because the corrected scores would then magnify the individual differences within the group. The magnification of individual differences is not genuine, however, because they are not necessarily true differences, the reliability of corrected scores actually being less than the reliability of uncorrected scores. It is possible that this increased variability is responsible for the more favorable showing of the right minus wrong scoring method with reference to validity.

ADDITIONAL RELIABILITY COEFFICIENTS

The data presented by Ruch and that presented in this paper are faulty in that the number of cases involved is rather small. For this reason, the writers obtained test blanks involving two duplicate forms of a same-opposites test given to 519 University of Minnesota sophomores by R. C. McKee in 1922–1923.[8] These two forms of the same-opposites tests were obtained by selecting two lists of 50 stimulus words from a list of 250 words which had been standardized to determine

[8] R. C. McKee, A Study of Controlled Association Technique. Unpublished M.A. Thesis deposited in the University of Minnesota Library, 1923.

the difficulty of each. These two forms, A and B, were scaled according to P.E. difficulty and given to 519 students who were instructed to guess. A short time limit was also imposed. McKee scored them according to the right minus wrong formula saying, "This procedure of scoring was used because it was possible to obtain a score of 50 per cent correct by chance. (See W. A. McCall, How to Measure in Education, pp. 127–128, for a full discussion of this scoring device.)" To utilize McKee's data for our present purposes necessitated rescoring all of the blanks, thus obtaining three scores for each student on Form A and on Form B. The three scores were the number right, the number wrong and the number right minus the number wrong. Table 5 presents the reliability coefficients computed for each of these methods of scoring, together with the means and standard deviations.

TABLE 5

Reliability coefficients for a same-opposites test according to the method of correlating scores in Form A with scores in equivalent Form B.

Number of cases equals 519

SCORING METHOD	r_{ab}	FORM A		FORM B	
		Mean	S.D.	Mean	S.D.
Number right	+0.69±0.016	38.0	5.5	38.0	5.5
Right minus wrong	+0.63±0.018	34.6	6.0	34.6	6.1
Number wrong	+0.43±0.024	3.9	2.5	4.0	2.7

Again, we note that the number right method of scoring yields a higher reliability coefficient than either the right minus wrong method or the number wrong method. The difference, as before, is not statistically significant even though the large number of cases makes the probable errors of the *r's* very small. The difference is actually less than three times the P.E. of the difference.

SUMMARY AND CONCLUSIONS

1. The reliability of a 100-item true-false test in two Psychology of Advertising classes was actually lessened when

the right minus wrong scoring formula was used to counteract the effect of guessing. The number right method was most reliable, the right minus wrong method was next, and the number wrong method was least reliable, although the differences are not statistically significant. These results hold for each class separately and for both classes combined, and are in direct agreement with Ruch's experimental findings.

2. Regardless of method of scoring, the reliability coefficients are low, throwing doubt on the true-false test as a universally reliable examining device.

3. Correction for chance by the right minus wrong method as compared with simply scoring the number right lowers the mean scores and increases the variability. This larger variability is not accompanied by higher reliability, hence it does not constitute any real gain.

4. The validity coefficients show no statistically significant differences. Though such differences as happen to exist seem to favor the right minus wrong method, yet scoring by number wrong is equally valid.

5. The validity coefficients, regardless of method of scoring, are low leading to the tentative conclusion that the true-false test is not universally a highly valid method of examination.

6. Additional data, giving reliability coefficients based upon the testing of 519 students with two duplicate forms of a same-opposites test, show that the number right method of scoring yields a higher reliability coefficient than either scoring right minus wrong or number wrong, although the differences are not statistically significant.

7. The above results corroborate Ruch's evidence at every point. No statistically significant difference appears in favor of the right minus wrong method of scoring. Indeed, the number right method actually produces higher reliability coefficients in every instance. It is more reliable regardless of whether the two-alternative response tests are given with or without a time limit, and regardless of whether the students are told to guess or are warned not to guess. These results are contrary to the assumptions which psychologists have so

confidently made in urging a right minus wrong scoring method in conjunction with two-alternative response tests.[9]

8. Incidentally, these investigations reveal the unreliability of the true-false, yes-no, or same-opposites test when only 50 items are used. Brown's prophecy formula applied to the data in this paper would show that at least 100 items need be used to produce a passably reliable two-alternative response test and that 200 items need be used to produce a thoroughly reliable test. Single-word-answer questions, completions, analogies and matching or pairing questions can be made to produce reliabilities of +0.85 to +0.90 much more economically. The writers predict that the two-alternative response test, with the right minus wrong dogma destroyed and with its reliability demonstrated to be frequently low, will be forced to yield to these other forms of new type examinations.

[9] One of the writers, in publishing material in advocacy of new type examining procedure, has been equally guilty of theoretically defending corrections for guessing without any such empirical evidence as is here presented. Henceforth, he will not insist on a right minus wrong formula and indeed will not even advocate the use of true-false tests unless they are multiplied to uneconomical limits. See Donald G. Paterson, Improving the Examination Function in Teaching, Bulletin of the University of Minnesota, Vol. XXVI, No. 31, August 4, 1923, pp. 47–56.

THE STATISTICAL VIEWPOINT IN VOCATIONAL SELECTION

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In a recent number of the JOURNAL OF APPLIED PSYCHOLOGY Viteles makes a plea for the clinical viewpoint in vocational selection.[1] He expresses the opinion that the statistical viewpoint is the representative one among research workers in selection and that this viewpoint, while appropriate to a technician, is not completely psychological. The points which he raises are of fundamental importance to experimenters in this field and deserve careful consideration by them.

The psychologist just as any scientist endeavors to collect a body of related facts established by scientific method. He relies essentially upon experiment to enlarge his body of knowledge. Because his science is comparatively young and has been subject to disputes on method the number of established facts is still not large.

Particularly in vocational selection where psychology made a rather late start have results been slow to accumulate. Nevertheless, by the use of the experimental method facts on vocational selection are gradually increasing. There is no other method which psychology can use to increase its body of knowledge in this field.

Let us review briefly the experimental approach to a problem in selection, in order to see the manner in which the statistical viewpoint operates.

The psychologist is usually called in when the employment department has failed to solve a definite problem such as the

[1] M. S. Viteles, The Clinical Viewpoint in Vocational Selection. Journal of Applied Psychology, 1925, Vol. IX, 131–138.

selection of the right men for a certain job. The management has heard that psychology is the science of human nature and what is more reasonable than for it to suppose that such a science can set it right. The system of hit-or-miss guesswork has failed and science is called upon to take its place.

Even the best psychologist cannot sit down at once and pick the right men for the job. More likely than not he knows nothing about the work. He does not know what abilities are necessary to succeed in it nor even what constitutes success in it. In consultation with the management he decides carefully upon a criterion of success in the job and selects excellent, fair, and poor workers for study. He analyzes the job carefully from every angle, observing the employees, and perhaps doing the work himself. He gradually accumulates material on the psychological, social, and economic aspects of the occupation, all with reference to the requirements for success in it.

The completion of this step still does not supply him with sufficient means for the selection of applicants. He must have some way of measuring the applicants with regard to the requirements for the job. For this purpose he constructs tests, rating scales, and questionnaires.

At this point the employment psychologist has two uncertainties. He must have doubts about the accuracy of his analysis of the requirements for the job and about the validity of his tests as measures of these requirements. He resolves both doubts by giving the tests to workers on whom he has some criterion of vocational success. He compares test scores with vocational success by statistical methods, minimizing the effect of disturbing variables, providing sufficient cases, and computing the error of his results. If he finds that a test correlates highly with vocational success he assumes that the close causal connection between the two variables shows that they call out the same abilities. Whether or not his original analysis of abilities was correct is not important to him now. Nor is it important to him whether or not his tests measure those particular abilities for which they were constructed. The tests

certainly measure some of the abilities necessary to success and his original analysis proved to be valuable in suggesting the kinds of tests to use.

The fact that careful statistical methods disclose a high relationship between test score and criterion warrants the use of the tests in selection for the job. Among applicants the known variable will be the test score and the predicted variable will be success at the job.

All assumed factors in success other than those represented in the tests are verified experimentally by rating scales and questionnaires and with the use of the same statistical methods as in the case of the tests.

Since these forms are standardized and quantitative they may be given by a trained examiner. He must be tactful and polite so that the worker is not led to believe that he is being put through a mill. The psychologist's further duties are to train the examiner, make certain from time to time that the examining methods are carried out as directed, and conduct further research with the purpose of bettering these methods and adapting them to changes in the conditions surrounding the job.

This in barest outline shows how the statistical viewpoint operates. It tries to put the selective process on a firm scientific basis. It tries to eliminate guesswork by providing objective proof to back every judgment. The examiner does not assume that he has any way of improving the selecting methods other than by experimental investigation.

The clinical point of view agrees with this up to a certain point. To quote from the article referred to:

> It is the opinion of the writer that in the cause of greater scientific accuracy in vocational selection in industry the statistical point of view must be supplemented by a clinical point of view. It must be recognized that the competency of the applicant for a great many jobs in industry, perhaps even for a majority of them, cannot be observed from an objective score any more than the ability of a child to profit from one or another kind of educational treatment can be observed from such a score. There is no reason for suspecting that the capacity of an individual motorman to avoid accidents, or of a printer's apprentice to profit

from instruction in this trade can be expressed in an objective score, as easily interpreted by a minor clerk as by a trained psychologist, than for suspecting that the mental status of a child is revealed in the I.Q. which can be obtained by any teacher who owns a copy of Terman's Condensed Guide and a set of testing material. The one problem is as complicated as the other; the objective score in one case has in it as many elements of error as in the other, and an adequate diagnosis in both involves interpretation by a trained psychologist based on observation of performance and a consideration of related data. . . . .

But industry must also be told with equal frankness that there are a great many jobs for which workers can not be adequately selected by tests administered and interpreted by employment clerks. It must be told that the selection of applicants for such jobs involves an examination by a trained psychologist who depends upon his scientific knowledge of human behavior as well as upon the test results. . . . .

This appears very much as if it were a case of changing horses in the middle of the stream. The psychologist starts out to measure the abilities required for the job. When he has evaluated statistically a series of tests he states that the tests are inadequate for the purpose and that the applicants can be selected properly only by a supplementary judgment of their performance which the psychologist alone by virtue of his special training is competent to give.

The field for the psychologist in industry is experimental research. If other fields seem greener they are a mirage. As soon as the psychologist forsakes his experimental results for his personal judgments he comes into competition with shrewd business men who have not had the benefit of laboratory training but have been forced into contact with all types and sorts of human beings. Is there any reason to believe that a psychologist can select men more accurately by adding his "scientific knowledge of human behavior" than a capable and experienced employment manager who is also in possession of the test results and is aware of their significance? The writer knows of no experimental data in support of this belief. There is plenty of evidence to show that psychologists do not agree among themselves in observing and judging human abilities even in employment procedure.

Let us see where the clinical viewpoint leads. If the psy-

chologist uses his trained judgment to interpret and supplement the tests and fails to select men accurately the results will be unfortunate to say the least. He will demonstrate his own inability to judge men correctly and will discredit the science as well. If he succeeds correct psychological procedure demands that he be guided by the statistical viewpoint.

If he succeeds in selecting men efficiently he either can or cannot point to the evidence upon which he based his correct decisions. If he cannot point to the facts then he has judged not as a psychologist but as a shrewd layman and neither the industry nor the science have gained by it.

Let us assume that he does point to certain evidence, other than test scores upon which he based his correct judgments. He observed that certain applicants were extremely nervous and upset while taking the tests and were barely able to restrain themselves from flying off the handle. He states that the observation of this fact as well as the test score accounted for his success in selecting men. If another made such an assertion the psychologist would be the first to demand proof of the validity of the procedure. He has shown exactly this attitude toward Dr. Blackford. While admitting her ability to judge men he has questioned her statement of the points on which she judges them.

The psychologist cannot point to the factors other than test scores upon which he based his correct judgments unless he keeps a record of his objective judgments on these factors and compares these records with the vocational success of the men judged. Thus he is forced to adopt the statistical viewpoint.

The psychologist's methods of judging these factors objectively can be incorporated into a rating scale and taught to a bright and sensible employment manager or examiner. The examiner does not have to know the entire field of psychology. He does have to know the technique in giving and scoring the particular tests and forms and the quantitative or qualitative observation of the other variables which the psychologist has proved to be important in selection.

If this procedure is carried out psychology is the gainer be-

cause facts are established and other research workers share information which otherwise would exist uncrystallized in the brain of one psychologist.

There is another form of judgment in selection where the clinical viewpoint at first glance seems to have a place. If the standings of the applicant in all the prognostic agencies are plotted the curve is commonly called the mental profile of the applicant. Presumably it becomes the function of the psychologist to diagnose the applicant's fitness for the job by observing this profile and weighing one ability against another. Anyone who has tried this method knows how difficult it is. There are innumerable kinds of profiles and in many cases the judgment of fitness is the wildest kind of guess. When statistical methods such as total correlation, critical scores, and weighting are in existence there is no justification for diagnosing mental profiles.

One of the assumptions of the statistical point of view, according to Viteles, is that of the relative unimportance of the individual worker in industry.

> A selective process which is satisfied with the probable adequate selection of a group of workers, rather than with the proper placement of the individual worker (and such is the case in selection based upon objective scores alone) fails to give adequate consideration to the well-being and interest of the individual worker, an interest which in comparison with the point of view of European psychologists, has been shamefully neglected by the American psychologist.

It seems to the writer that the statistical viewpoint does not suffer by comparison here. The discussion concerns vocational selection and not vocational guidance. The employment office is aware of every vacancy in the company and can test the applicant's fitness for each of them. If he fails to measure up to the standards of any of the jobs the company cannot be interested in him any further. The clinical viewpoint can add nothing to this. In fact it may be ventured that the clinical viewpoint by allowing selection to be influenced by personal interpretations with their unavoidable prejudices instead of relying upon objective measures gives even less consideration to the well-being and interest of the individual worker.

The writer is not in disagreement with the clinical point of view if it means that social, personal history, and economic information should receive the same consideration as psychological abilities. These very important facts have been unaccountably neglected by research workers in spite of the fact that in many instances of their use, notably with life insurance salesmen, they have proved to be important. When they are studied they should be evaluated in the same statistical way as the tests. They can then be incorporated into questionnaires and rating scales and determined by the trained examiner.

There are some fields of psychological activity where the clinical method is justified. Vocational guidance as distinct from vocational selection is one of these. The difficulties in the way of checking the accuracy of vocational guidance are almost insurmountable. Progress must wait on the establishment of scientific facts in vocational selection in order that these may be applied in guidance. Meanwhile there are some proved measures as well as social and economic facts for the use of the psychologist and for the rest it may be granted that his advice will be as trustworthy as any other influence in motivating the youth to go into one or another vocation. But vocational selection offers a comparatively clear experimental problem with an opportunity for verification of judgments and when this opportunity exists there is no justification for adhering continuously to the clinical point of view.

The same writer upholds the clinical viewpoint in another recent paper.[2] Here he presents a number of principles "fundamental to the use of tests in the adequate guidance of the individual." We are interested in only four of these principles which are given in the author's words.

(4) *The factors which determine vocational competency are the individual specific mental abilities. For positive guidance the specific abilities of importance in the various occupations must be subjected to measurement.*

. . . .

(5) *Tests, to be of service in guidance, must measure not only the general*

[2] M. S. Viteles, Psychological Tests in Guidance: Their Use and Abuse, School and Society, 1925, Vol. XXII, 350–356.

and specific mental abilities which are of importance in vocational success; tests must also be provided to measure the temperamental and character qualities which play so great a part in vocational adjustment. . . . .

(6) *In the examination of the individual for the purposes of guidance the observation and interpretation of the quality of the performance is at least as important and in many cases more important than the quantitative score.* The specific abilities and the specific skills referred to in the discussion of the preceding principle are in many cases lost in the quantitative statement of accomplishment on a test. The degree to which they are to be found in the individual is revealed in the quality of his performance, which can only be seen through careful observation by a trained examiner. . . . .

(9) *The validity of a test or of a battery of tests as measures of a trait or traits significant in guidance can only be determined by a correlation of test results with "an adequate criterion of ability to succeed in the work in question."*

This illustrates the logical contradictions into which the clinical point of view leads. Every ability of vocational importance must be measured by tests constructed for the purpose and yet some of these abilities cannot be measured by tests. The validity of each test as a measure of a certain ability can only be determined by correlating the test results with a criterion and yet the assumption is made without resorting to such correlation that the test score is not an index of the ability and that the extent of the ability is revealed direct to the psychologist.

THE PREDICTION OF A DISPARITY · BETWEEN SCHOLARSHIP AND INTELLIGENCE

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The problem of predicting special traits requires first that the special trait shall be defined if possible in objective form. The trait here studied is that of the disparity between intelligence test records and scholarship for a group of 456 college students whose scholarship records were available for the first two semesters, and who had taken the Army Alpha Test. Disparity was measured by the difference between the records of a student in terms of the standard deviation of the Alpha and of the scholarship score for each semester. So far as the trait of disparity is concerned the group was divided into those students who were higher in scholarship than the intelligence test record would anticipate, and those who were lower. The scholarship-higher and the scholarship-lower groups were then each divided as to the degree in which they showed the trait. Three degrees in each direction were defined: First, the extreme disparity cases, those who showed consistently for two semesters a disparity of at least one time the standard deviation, or one sigma; second, the medium cases, those who showed consistently at least 0.5 sigma but not consistently over one sigma; third, the slight disparity cases, those who showed consistently some disparity but less than 0.5 for at least one semester. The students who were inconsistent in the direction of their disparity, for the two semesters (105 in number), were classified on the basis of their average disparity for the two semesters.

An important practical problem is to find the students at the beginning of their college course who will be likely to do work far below their intellectual capacity. The studies made

by Stone,[1] by Laird,[2] and by Poffenberger and Carpenter,[3] and also the present study fail to solve this practical problem. A simple way to explore the situation for leads is to use scatter diagrams in the form of correlation tables for a random group for the tests which seem best to separate the medians of the extreme groups. The different degrees of the trait may be shown by different colors of ink. Borderlines may be drawn which seem best to set off the cases which it is desired to select. Various combinations of the best tests thus found may then be tried. At first it is better to work with a small exploratory group of say 50 cases. The solution of the problem would ultimately require weighting the tests thus discovered by a partial regressive equation. Much preliminary work promises to be necessary before such refinements need be tried.

Let us first take up the results from trying to predict disparity by the total Alpha score and by the separate scores in each of the eight tests of that series. This is the only random group studied which contained the whole range of disparaties and which was large enough to try out the method thoroughly. It is essential to use a random group in order to apply the results to such a practical problem as selecting extreme cases of a trait. This should be emphasized since much time may be wasted on similar problems by attempting to discover borderlines for extreme groups by tabulating only cases showing the trait in extreme degrees. For example, if one plotted only the extreme scholarship-higher and extreme scholarship-lower groups and determined borderlines which definitely separated these groups, these borderlines would not serve to pick out the extreme cases from the rest of a random group who might have identical scores on the tests. The real problem is to find the cases of extreme disparity. When groups

[1] Stone, C. L., Disparity between Intelligence and Scholarship, Jour. of Educ. Psychol., 1922, XIII, 241–244.

[2] Laird, Donald A., A study of Some Factors Causing a Disparity between Intelligence and Scholarship in College Students, Sch. and Soc., XIX, 290–298.

[3] Poffenberger, J. T., and Carpenter, F. L., Character Traits in School Success, Jour. of Exper. Psychol., 1924, VII, 67–74.

showing only extreme and opposite degrees of a trait are used they serve only to test whether a series of borderlines will distinguish these extreme groups from each other, not whether they will separate the extreme cases from others. The results with the Downey Will-Temperment Tests, which will be reported later, were obtained only for extreme disparity cases. They, therefore, serve only to check types of profiles which might be thought to predict disparity. On the other hand if extreme disparity cases could be found with any combination of the separate Alpha Tests the method may be trusted to have discovered such borderlines.

A word should first be said about the general relations between the Alpha Test scores and scholarship at Kentucky, since these indicate the reliability of the measures used. Credit for gathering the data and for calculating the correlations in this paper should go to E. J. Asher. His master's thesis included also systematic interviews and the individual Downey Test profiles for over fifty extreme disparity cases. For the 456 students in the College of Arts and Sciences studied in this paper the correlations between test and scholarship score were 0.44 for the first semester, 0.40 for the second semester. The reliability of the scholarship score is indicated by a correlation of 0.67 between scholarship for the first and for the second semesters. Most important in the study of disparity was the fact that disparity for the first and second semesters correlated 0.45, so that the records were undoubtedly getting some sort of complex trait which did not disappear after a semester in college. This was true for the group as a whole even when the records had not been corrected by a second intelligence test. Such verification of disparity is undoubtedly important since it was found that about one in five cases dropped out from the extreme disparity groups where the disparity was checked by a second intelligence test. The absence of verification by a second test record accounts easily for the reliability coefficient for disparity not being over 0.45 with this larger group.

The first suggestion which was tried out was that the variability of the students from intelligence test to scholarship

might be an indication of a fundamental variability which would show itself also in the variability of the same student from test to test in the Alpha series. This was examined by taking the inter-quartile difference in sigma scores for the eight Alpha Tests as a measure of each student's variability and correlating this with the disparity between total test score and scholarship. Individual variability on the eight tests of the Alpha series proved to be no measure at all of disparity. The correlation with disparity for the first semester was -0.05, and for the second semester -0.02.

Taking up next the prediction of the direction of disparity by means of scatter diagrams, from a sample group of fifty cases, the two best tests for discriminating the direction of disparity were found to be Test 5, Mixed Up Sentences, and Test 8, Information. Regarding as favorable for scholarship relative to tested intelligence a score below 18 on Test 5, and below 24 on Test 8, and as unfavorable a score of 18 or above on Test 5, it was possible to properly place, as to the *direction of disparity*, 30 out of 35 cases which fell outside these limits. This percentage of success was thus 81 per cent. When these criteria were checked for the 289 cases outside these borders among the 451 cases, the number of correct predictions was found to be 78 per cent. The chances were thus 4 to 1 that the direction of disparity could be predicted for cases falling beyond certain borderlines. Supplementary records for the other Alpha Tests and the total Alpha scores were hardly worth while for the prediction of the direction of disparity.

It is a much more difficult problem to pick the cases that may be expected to show an extreme disparity of at least one sigma for two semesters. This is, however, the main practical problem, since a less disparity cannot be assumed to indicate a student who should be dealt with individually. The attempt to find extreme cases was made only for the scholarship-lower group. Here the best criterion proved to be a score of plus one or more sigma for the total Alpha score as likely to go with extreme scholarship-lower. There were 72 cases beyond this borderline among the 456. Seventy-five per cent of those 72

cases would be rightly predicted as to the direction of disparity, 55 per cent as showing at least medium disparity (0.5 sigma or more); but only 32 per cent of them were of the extreme degree of disparity sought, that is, of the type for whom special training would be clearly worth while. It is also to be noted that this borderline failed to find 17 out of 40 cases with extreme-scholarship-lower even if we might deal with those which it did find. When the above borderline for the total Alpha score is supplemented by the two best individual tests for distinguishing scholarship-lower, Tests 5 and 8, the situation is only slightly improved. Reducing the 72 cases to 31 who are also above scores of 20 on Test 5 and above 26 on Test 8, we find that the chances are 23 to 8 that such a case will show at least medium scholarship-lower, a disparity of 0.5 sigma or more. For these 31 cases, however, the chances are 19 to 12 that they will not show a disparity of 1 sigma. Under the most favorable determination of borderlines, with the chances still approximately 2 to 1 against a case being of the extreme disparity sought for special training, it is clear that other types of tests than those in Alpha should be tried.

Since the Downey Will-Temperament Tests apparently afforded one of the best instruments yet devised for getting objective records of a variety of personal traits, it was decided to try them with very carefully selected groups of extreme disparity. To be a member of these groups the student must have been tested by a second intelligence test; second, have consistently maintained a disparity of at least one sigma for each semester; and, third, be available for an interview and an individual test with the Downey series. Only 32 cases were found which met these requirements. They are much more rigid than have been applied by others who have studied the problem.

By inspection it did not seem possible to distinguish any typical differences in the general profiles even for these extremely different groups, 16 with extreme and consistent scholarship-lower, and 16 with corresponding scholarship-higher than tested intelligence. However, it might be that Dr.

Downey, who has worked so long with such profiles might find patterns in them which escaped other observers. She very generously consented to attempt to place these 32 cases in two groups: Those from whom she would expect scholarship lower than tested intelligence and those from whom she would expect scholarship higher. In returning her conclusions she qualified them by the statement that her judgment was based largely upon unpublished work which she had begun on the problem but in which she had used the group test rather than the individual test. She did not know whether such results would carry over to the individual Will-Temperament Test. She also supplemented her judgments by qualifications as to whether the student had a high or low record in tested general intelligence. It is obvious that the Downey Tests were not devised with such a problem in mind, so that it would be wholly unfair to regard this trial as a test of the Will Temperament diagnosis for other purposes or of the Downey group test. Too much credit can hardly be given Dr. Downey for her pioneer work in this fascinating field.

Considering Dr. Downey's conclusions as to which of these two extremely opposite groups a case belonged, both with and without her allowances for intelligence test records, her guesses proved to be right in just half the cases, just what a random sorting of the cases would have given. This may mean only that criteria from the group Will-Temperament Test should not be transferred to the individual Will-Temperament Test for this problem.

Knowing in which group each of these 32 extreme disparity cases belongs it is a comparatively easy matter to discover borderlines on scatter diagrams which will separate them all properly. The simplest criterion was found to be the total Alpha score, the extreme scholarship-higher students in this group with one exception were all below a score of 126 on Alpha. With a single exception also, those with scholarship-lower scored above 141 on Alpha. The two exceptions, between 126 and 140 on Army Alpha may be properly classified by being above and below the median score of 5 on the Downey Speed of

Movement Test. The two Downey Tests which seem best to discriminate these extreme disparity cases are Speed of Decision and Motor Inhibition, but combined they fail to reach 7 of the 32 cases.

The important conclusion to emphasize is that test records on extremely different groups are of little significance until comparable records are available for a random group of students. The above tests and borderlines must not be used to predict disparity since many other students with the same scores would show little disparity or an opposite tendency. Work with extreme cases alone can never solve the problem of predicting extreme cases of any type. This is demonstrated by the fact that the Alpha total score would easily separate 30 out of the 32 extreme disparity cases; but if these borderlines were used to predict extreme disparity, they would be wrong for four out of five cases in our large random freshmen group beyond the same borderlines. The borderlines which successfully separated the extreme cases are apparently those which distinguish very good test records from very poor records, and therefore leave the biggest opportunity for a disparity from scholarship.

It is of interest to note that the interviews by Mr. Asher were directed to finding, if possible, what seemed to be the main reasons for the extreme disparity. The results, as subjectively evaluated by him, indicated, for the 27 extreme scholarship-lower students on the basis of their first semester scholarship, that the primary influence was apparently not environmental. For 11 cases it was lack of interest in the college work, for 6 slow adjustment to the new environment, for 5 other character defects of various kinds, and for only 5 was it unusual outside influences or illness. For extreme and persistent cases, it would, therefore, seem that scholarship-lower is based on combinations of personal traits which should be discoverable by tests.

A STUDY OF THE COMPARATIVE VALUE OF NINE PERFORMANCE TESTS

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Performance tests, from the Pintner Paterson series, are relied on very largely in determining the intelligence of non-English-speaking persons. Frequently, due to lack of time or material, a few only of such tests are relied upon.

Does it make a great deal of difference which ones are used? Do some have a constant tendency to yield mental ages higher or lower than the median mental age which all of them if used, would yield? Are some more variable than others in comparison with such median mental ages?

This study helps partially to answer these questions for nine of these tests. The results from them are compared not with the median mental ages which the use of all the series would produce, but with the medians obtained from the use of only these nine tests taken together.

The nine tests are: Mare and Foal, Seguin, Five Figure Board, Two Figure Board, Casuist Form Board, Manikin Test, Feature Profile Test, Picture Completion and Cube Test. Time, not errors, was scored for those tests where scores can be arrived at in either way. The test materials were obtained from Stoelting and Company.

These tests were all given to a group of 29 young unmarried mothers at the San Antonio Mission Home and Training School by the writer and one other psychologist and an assistant, in the course of the Texas Mental Hygiene Survey which was conducted by the National Committee for Mental Hygiene in the fall of 1924. The method of giving and scoring the tests described in "A Scale of Performance Tests" by Pintner and Paterson, was followed. Half of the tests

were in charge of one psychologist, and half in charge of the other. Half of the subjects were examined first by one of these psychologists and half by the other. One of the psychologists rotated the order of giving the tests she used; the other one did not. The Two Figure Form Board thus came to be given first of all the tests, to half of the subjects and the Casuist Form Board second. This latter and the Manikin, Feature Profile, and Cube Tests never were given first. Each of the other tests was given first to about one-eighth of the subjects.

The median mental age from all the nine tests was calculated for each subject and the deviation from this of the mental age determination of each test. The average of these

TABLE 1

	MARE AND FOAL TEST	SEGUIN	FIVE FIGURE BOARD	TWO FIGURE BOARD	CASUIST FORM BOARD	MANIKIN TEST	FEATURE PROFILE TEST	PICTURE COMPLETION	CUBE TEST
Average deviation from median M.A.	+1.89	−2.69	+0.45	+0.65	+1.41	−4.90	+1.31	−0.41	−2.27
Average amount of deviation, up and down from median M.A.	2.31	2.83	1.41	1.72	2.31	4.90	2.14	1.45	2.62

deviations taken algebraically appears in the first row of the accompanying table. This shows, for each of these nine tests, how far it tends to over- or under-estimate the mental age, as this is determined by all of them. Thus we see that the Mare and Foal Test tends to give a mental age 1.89 years too high, the Seguin Board, one 2.69 years too low, the Five Figure Board, one only +0.45 of a year away from the median, etc. Of the nine tests studied, the Five Figure and Two Figure Form Boards and the Picture Completion Test yield the best results for subjects of this mental age (average for the group, 11.7 years, σ2.31). Mare and Foal, Casuist Form Board and Feature Profile give over age results; Seguin Form Board, Manikin, and Cube Test all decidedly underrate the subjects.

The reliability of the results just quoted is measured by the coefficient 0.89. This is the correlation obtained for the nine tests between the sum of the scores of 15 of the subjects and the scores of the other 14, by the method of difference in relative position.

The relative variability of scores by the nine tests from the median mental age, is shown in the second row of table 1. It is the average for each test of the amount of its deviations from the median mental ages, taken arithmetically. It shows that the Mare and Foal Test varies on the average up or down, 2.31 years from the median mental age; the Seguin Test varies 2.83 years; the Five Figure Board varies 1.41 years, etc. The reliability of these results is not very great, the coefficient of correlation between the results on fifteen subjects and on the other fourteen is 0.42 only. They indicate, however, that the three tests, Five and Two Figure Board and Picture Completion, which are best of the nine because they average nearer the median mental age, are best also because they show less variability in any direction from the median than do the other six tests.

MINOR STUDIES IN THE PSYCHOLOGY OF ADVERTISING FROM THE LABORATORY OF INDIANA UNIVERSITY

XIII. Pictures of People in Magazine Advertising

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Much is said in the literature on the technique of advertising about the value of injecting "human interest" into advertising copy. One way of doing this which is recommended is to use pictures of people. An attempt to measure the value of these was made by Nixon[1] who, working under laboratory conditions, found that his subjects looked at advertisements containing people more often than they looked at advertisements containing pictures of objects, and that they gazed at the former for longer periods of time; in one series of experiments 11 per cent longer, on the average, and in another series, 25 per cent.

In order to find out to what extent advertisers are using and have been using this device, investigation by the historical method is required. No such investigation is on record, though the casual observation is made by Nixon that "a survey of current magazines shows that over 65 per cent of full page magazine advertisements contain pictures of humans as a prominent feature, and it is probable that this use is on the increase." In order to gather exact information on this point the writers examined 500 consecutive illustrated ad-

[1] Nixon, H. K., Attention and Interest in Advertising, Archives of Psychology, Columbia University, New York, 1924.

Nixon, H. K., Two Studies of Attention to Advertisements, Jour. of Applied Psychol., June, 1925, IX, 176–187.

vertisements (all sizes) in each of the four periodicals of the issues shown below:

Saturday Evening Post, April 18, and May 2, 1925.
Literary Digest, March 14 and 21; April 4, 11 and 18; May 2 and 9, 1925.
Womans Home Companion, May and June, 1924; January and May, 1925.

In order to discover the trend over a lapse of time, 500 consecutive illustrated advertisements were examined in past issues of the *Literary Digest* as follows:

From July 3 to September 4, 1915
From July 7 to October 13, 1906

TABLE 1

Frequency with which humans appear in illustrated advertisements

MAGAZINE	TOTAL NUMBER ILLUSTRATED ADS EXAMINED	NUMBER SHOWING MALES	NUMBER SHOWING FEMALES	TOTAL NUMBER SHOWING HUMANS	PER CENT SHOWING HUMANS	PER CENT (OF LATTER) SHOWING MALES	PER CENT SHOWING FEMALES
Saturday Evening Post.....	532	306	216	384	72.2	79.7	56.2
Woman's Home Companion.....	500	141	328	373	74.6	37.8	87.6
Literary Digest.....	513	249	187	322	62.8	77.3	58.0
Literary Digest, 1915.....	500	237	108	274	54.8	86.5	39.4
Literary Digest, 1906.....	500	152	94	219	43.8	69.4	42.9

Table 1 shows a frequency of pictures of human beings in the illustrated advertisements examined as represented by the following per cents: *Saturday Evening Post*, 72; *Literary Digest*, 62; *Woman Home Companion* 74. The average frequency of all is 69.

The data were so tabulated as to show the number of advertisements containing pictures of males and the number contain-

ing pictures of females. (Some contained both.) The per cent showing males exceeds the per cent showing females in both the *Saturday Evening Post* and the *Literary Digest*, the figures being 79 per cent males and 56 per cent females in the former, and 77 per cent males and 58 per cent females in the latter. This may mean that the majority of the readers of these periodicals are men; or it may mean that the articles advertised in these periodicals are predominantly those used by men.

In the woman's magazine, as would be expected, the number of advertisements containing pictures of women is greater, the figures being: females 87, males 37. The trend of practise during the past twenty years is indicated by figures gathered from a study of the files of the *Literary Digest* for 1906 and 1915. These figures show that the use of pictures of humans has been increasing during the past twenty years. In 1906 the per cent was 43; in 1915, 54; and 1925, 62; an increase during the past twenty years amounting to about 40 per cent.

It is interesting to note in this connection that the use of pictures of women has increased more rapidly than the use of pictures of men. In 1906 the number showing females was 42, while in 1925 it reached 58, an increase amounting to 38 per cent. In 1906 the number showing males was 69, in 1925, 77, an increase amounting to only 11 per cent. This increasing popularity of pictures of the female may indicate several things: That the proportion of women readers of the *Literary Digest* has increased; that an increasing number of commodities used by women has come to be advertised in this periodical; that the number of women buyers (wage-earners?) has increased relatively faster than the number of men buyers; that it is coming to be recognized by advertisers that women do most of the buying. One might venture the hypothesis also that perhaps advertisers have been trying to use the much flaunted "sex-appeal" among the male readers of this masculine (?) periodical. To what extent each of these factors contributes to the increase in the use of pictures of females can only be a matter of conjecture.

The tendency toward use of pictures of humans in advertis-

ing copy should be viewed in connection with another fact. It has already been discovered by one of the writers that there is a growing tendency in advertising copy to describe an article in terms of its use.[2] In carrying this out the most natural medium to employ is a picture of a human being using the article.

Another factor may be simply the well-known increase that has occurred in the absolute number of illustrations. Figures already published by one of the writers have shown that the frequency of illustrations rose from 70 in 100 to 90 in 100 during the period 1907 to 1919.[3] With this increase in number together with the great advances made in the technique of illustration, would naturally go a wider use of human materials.

SUMMARY

This investigation has shown that in the representative magazines examined the number of illustrated advertisements containing pictures of humans is on the average 69. The figures show that the tendency in this direction has been increasing during the past twenty years, the increase amounting to about 44 per cent. In the woman's magazine examined the advertisements containing pictures of women predominated; in the two general magazines, the number containing pictures of men predominated. Pictures of women have been growing most rapidly in popularity.

[2] Kitson, H. D., The Mind of the Buyer, New York, Macmillan, 1921, 83 ff.

[3] Kitson, H. D., Amount and Rate of Increase in the Use of Illustrations, Journal of Applied Psychology, March, 1921, V, 12–13.

A SPEECH CLINIC CASE WITH MISCONDUCT AS A BY-PRODUCT

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Wilma was brought to the clinical psychology class for examination because of her inability to speak properly and because she was a disciplinary problem both at home and at school. The arrangements for her examination were made by a student of the class who had been her teacher in the 1A grade. Although Wilma was a girl seven years and three months old and in the 2B grade of the Indianapolis Public Schools her teacher doubted if it would be of much value to the class to be present during her examination because her speech was so poor no one could understand more than a very small percentage of all the words she spoke. As it is the purpose and obligation of clinical psychology to study all types of cases, the teacher was advised to bring the girl.

The teacher reported that she had promoted Wilma to the second grade because she felt she could do the work. Her written work and comprehension were very good. She was liked by everyone. She had been put into an atypical class for reading each day on account of her speech. She hated this period. The children in their play seemed to understand her. She liked to play games out of doors. In the school room she had a horrid disposition, was a vicious child seemed possessed to pinch and kick somebody. She pouted, was not happy and was never amused at anything that ever happened in the school room.

Wilma was brought to the clinic by an aunt who reported that when Wilma was three years old while riding in an automobile with her family a train hit the auto and killed all other members of her immediate family. She was thrown a dis-

tance of about 100 feet and was considerably bruised but had no serious injuries. After this accident she was taken into the home of this aunt and has been with her ever since. The girl had never been able to talk distinctly. The aunt says that she had tried every means she could think of to help the girl but had not been able to produce any effects upon her speech. Two different physicians had been consulted. One told the aunt that the girl's speech was a matter of habit, that there was absolutely nothing wrong with the child and that she would learn to speak properly when she went to school. The other said that the girl could never be expected to speak properly because for some reason the nerve of speech was undeveloped. At the time Wilma entered school the aunt explained her defective speech to the principal and teacher stating that she doubted the value of sending her to school because no one could understand her. The principal informed the aunt that they frequently get children with speech defects and that they always work with them and teach them to speak properly in addition to giving them the regular school work. After that they get along satisfactorily. Such was the encouragement given the aunt in February, 1924, when Wilma entered school. At the time she was brought for examination in February, 1925, a year later, both the principal and the first grade teachers reported that they could not teach the child to speak properly. The principal said this is the first child upon whom they have failed entirely. Although Wilma was promoted to the second grade the general impression seemed to be that her speech was very little if any better than when she entered the first grade.

The aunt hesitated to bring the girl to the clinic because she herself had tried various ways and means of teaching the girl and had received no satisfaction, and because even after the encouragement given her by the school principal, Wilma made so little improvement that the aunt was inclined to feel nothing could be done for her speech. Some people had told her she should not pay any attention to it and that the child would outgrow it. The aunt felt she had waited long

enough. She had about concluded that the child could not be instructed and that she would not outgrow it. She said, "Not one single word of her reading could be understood." Wilma could not say many of the ordinary things around home without much explanation. By way of illustration the aunt told of one occasion upon which Wilma wanted to tell her something was "yellow." No one at her home could understand her. Finally she told them it is like the sun. In all, it required half an hour for her to succeed in having the family understand that she meant to say "yellow." She pronounced her own name and address so imperfectly that had she gotten lost from home it would have been impossible for her to make a stranger understand who she is or where she lives.

In addition to the girl's defective speech the aunt was very much worried because of her behavior. The girl's general attitude and conduct were a source of as much worry as that of her speech. In fact, it seemed to impress people as the more serious of the two.

PSYCHOLOGICAL EXAMINATION

Wilma first met the examiner in his class in clinical psychology. The entire examination was conducted in front of the class. She was given the Stanford Revision of the Binet test together with performance tests to determine her general mental level and to get an index of her powers of observation, attention, imageability, attention span, and ability to learn. At the conclusion of this examination she was diagnosed as of slightly above average normal mental ability. Her mental age on the Binet was seven years and six months or three months above her chronological age. In making this diagnosis considerable weight was given to the quality of her performances on the performance tests. This was necessary because her answers to some of the Binet tests were difficult to understand while in other cases it was impossible to decide definitely what she was attempting to say. At times it required the best efforts of the class, the aunt, and the examiner to interpret what she might have meant by the sounds she

made. Often she was asked to repeat her statements three and four times before any one of those present felt safe to venture a guess.

After the above tests were completed the examiner had Wilma say the sounds: "ah," "ee," "oh," "oo," and that of the pronounciation of several simple words. This constituted the entire examination.

During the entire period of the above examination, Wilma's speech was studied very carefully by the examiner although unknown to Wilma or the class. This study was directed to her speech in an effort to determine whether there might be any physical condition responsible for her speech. Such physical condition might consist of malformations of such a nature that it would be impossible to utter the sounds essential in speaking English. Or they might consist of paralyses which made it impossible for Wilma to control the organs of speech in attempting to pronounce words. Keeping these possibilities in mind the examiner noted that Wilma apparently was able to control the sounds essential to the speaking of good English but did not have these properly coördinated so that the language she spoke was English. This convinced the examiner (1) that there was no serious physical abnormality present, (2) that her unintelligible speech was the result of improper speech habits and (3) that if the known laws of learning and habit formation were intelligently applied in her case she could easily learn to speak correct English. For this reason the examiner predicted that if Wilma were taught with these objectives in mind and consistently employed that a 100 per cent cure could be effected within a year or a year and a half. When the examiner informed the aunt of his conclusions she did not appear very much impressed by them and gave the attitude that she had heard such encouragements previously and that the examiner would later learn of his erroneous conclusions. She was, however, willing that arrangements be made for the girl to be taught under the examiner's direction.

The success in a case like this depends upon the ability of

the instructor to make a correct diagnosis of the condition and then to direct the overcoming of a wrong set of habits and the building up of a correct set by direct and easy guidance of the learning processes. To do this a new diagnosis must be made at each stage of the successive changes through which the individual being taught must go in making the transition from the old to the new habits. This means that no cut and dried method is applicable and that the effectiveness with which success is achieved will depend upon the teacher's ability to make accurate detailed observations and correct interpretations of these observations. It also means that the teacher must have such a knowledge of how old habits are displaced and new ones formed that it functions in the directing of the teaching process as indicated by the interpretation of the observations made.

Because it was impossible to secure a teacher with training in corrective speech work it was necessary for the examiner to select an untrained person and direct the work. Miss Dorothy G. Daugherty, a graduate student in clinical psychology, was selected because of her training in clinical psychology and her acquaintance with the fundamental laws of learning and habit formation. It is interesting to note the fact that throughout her college course Miss Daugherty had intentionally avoided all education courses because she was determined never to teach. When Wilma's case was explained to her she agreed to undertake it but made it clear that she had never intended to teach and had never given any attention to teaching methods and practice.

After Miss Daugherty had agreed to teach Wilma and Wilma's aunt had also agreed to the teaching, the aunt seemed to feel that even though success was unlikely Wilma's speech would be given due attention. According to the plan Wilma got from one-half to three-quarters of an hour instruction every Saturday morning from February to June, 1925. Lest the examiner had forgotten about Wilma's conduct, the aunt asked about suggestions for her discipline. The examiner advised her in as strong terms as possible to "forget it," and

then explained that the speech was the primary condition to be considered and that Wilma's conduct might cease to be a problem providing her speech is corrected.

The aunt was naturally concerned about Wilma's behavior because many people blamed her for not disciplining her properly, while other people felt that Wilma was an abnormal child. The aunt reported that various forms of punishment had been tried, but in no case was Wilma whipped. Wilma was in trouble in school so frequently that reports of her bad conduct were sent home on an average of about once a week. At times during school hours she would just make loud, ugly noises.

The week after the clinical examination and diagnosis were made both Wilma and her new teacher in speech appeared for their first lesson. One-half hour of time was spent by the examiner on giving Wilma her first speech lesson and instructing her teacher how to continue the lessons. As different aspects of the problem were revealed the examiner explained what he considered to be the fundamental principles to both pupil and teacher. After this first lesson the examiner did not spend more than fifteen or twenty minutes with Wilma. During this first lesson at least two significant features of the case were brought out. It was found that "chair" was one of the words Wilma could not pronounce intelligibly. It was further found that she could with very little practice give the "ch" sound and the "air" sound, each independent of the other but could not possibly combine them into the word "chair." She was therefore induced to pronounce both these separate sounds as independent words, decreasing the time elapsing between the pronunciation of the two parts of the word. She was, however, never able to pronounce the word during this first half hour. Whenever she was asked to say "chair" she invariably pronounced it by her old method. This is illustrative of her difficulties. During this first lesson she was asked for a word which she could not pronounce. In reply she tried to pronounce a phrase which she pronounced so poorly that her aunt could understand it only after several

repetitions. "Thank you" was the phrase she was attempting to say and was very anxious to learn correctly. During this first half hour we were unable to teach her to say "thank" but the examiner carefully told her how to place her tongue and other speech organs. Then by way of a side remark the examiner told the aunt that the best thing for Wilma to do would be to practice that during the week and if possible to practice it in front of a mirror where she might observe her own movements. That this method of trying to spur Wilma on succeeded was evidenced a week later when she appeared for her lesson and could say "thank you" very well.

Wilma took hold of the work so actively that the examiner realized she would make very rapid progress, in fact would probably make much more rapid progress than would ordinarily be expected. For this reason a sample of her reading was taken upon her fourth visit to the Clinic. By this time she had already made sufficient improvement to pronounce correctly some of the words which she had been unable to pronounce previously, that the sample of her reading taken at this time is not as poor as it would have been had it been taken upon her first visit to the Clinic. The aunt stated that before Wilma came to the Clinic no one could understand a single word of anything she read aloud. A copy of the passages to be read and an attempt at a spelling of the words as they sounded to a listener when she pronounced them are quoted below one above the other. Even though her pronunciations were incorrect she was able to understand and interpret everything she read.

Original: "A long time ago a dog had the right to eat
Wilma's reading: "A om ti ago a dodge ad de wite to eat

all the meat that fell on the floor. That was a fine time for the dog.
all de meat at fall on the for . That wa a fi time far de dodge.

He was fat and happy.
E wa fa an appy.

"He was afraid someone would take this right from him. So he
" E wa afade sahvon vood take thit wite from eem. Szo e

asked the king to give him a paper and the paper said that the
ak de kid to gi eem a baber an de baber sai at de

dog could have the meat. The cat said she would keep the paper
dodge coo ave de meat. The ca sai se vood teep de baber

for the dog."
far de dodge."

Five in One Pod

"There were once 5 peas in one pod. They were green and the
"There war once 5 bae in bon bod. Ze var feen and de

pod was green. So they thought all the world was green. Why
bod vor feen. Zo de taut off de worl wa feen. Vy

shouldn't they have thought so? The pod grew and grew as they sat
sooda ay ave taut so? The bod brew and brew as ze gat

there in a row."
there in a row."

On the same day that this sample of reading was secured Wilma was asked to go before the class in Clinical Psychology again and read for them. At first she refused to go. She went only after considerable urging in which it was explained to her that the purpose was for her to let them hear how she reads now and then later when she can read properly she could go before them again to show them how much she had improved. Three months later on the next to her last trip to the Clinic she asked permission from Miss Daugherty to go in and read before the class. This is an indication of the change in the girl's attitude toward life in general. Another indication of this change in attitude is the fact that the school did not report her to her aunt for bad conduct during the last two months of the school year. Prior to that a report for bad conduct had been sent to her home on an average of about once a week. The aunt estimated that at least 75 per cent of the causes of worry about Wilma's bad conduct had disappeared.

The teacher reported she was much happier in school and that now she wanted to talk to the teacher and help her. She never wanted to talk or help before. When asked why she

behaves so much better now than she used to, she said, "Guess 'cause I wanted to be naughty," but she could not tell why she wanted to be naughty before and does not want to be naughty now.

The great improvement which Wilma made in her speech during the four months from February to June is indicated (1) by the aunt's statement, that "There isn't anybody who has any trouble understanding her now," and (2) that whereas in the two passages of 118 words quoted above she pronounced only 30 words or 25 per cent intelligibly late in February, she pronounced 111 words or 93 per cent intelligibly early in June. The only words she mispronounced entirely on this latter date were "had," "fell," "his," and "all." She had considerable difficulty with "right," "world," and "why." Her improvement in conversation was greater than that in reading aloud.

It is not known exactly why Wilma did not speak correct English. Indications are that she developed an individual pronunciation of English meaning words through contact with Polish servants in her home when she was a child. Her pronunciations, however, did not impress one as that of a foreigner trying to learn English. They seemed more like that of an individual language. From that angle it hardly seems fair to the facts to speak of her as a speech defective, unless we wish to classify all users of other languages as speech defectives.

Her rapid acquiring of the English language was due to several facts.

1. That she had English meanings and comprehension, but imperfect English pronunciations.

2. That her condition was correctly diagnosed at the beginning and correctly re-diagnosed at every step of instruction.

3. That she was therefore instructed from the beginning according to known laws of learning and of habit formation with the instruction always aimed directly at the needs indicated by the re-diagnoses of her progressively improving English speech.

4. That she was of normal mental ability somewhat above average.

5. That she was so conscious and painfully aware of her unintelligible speech that she came with an intense desire to learn to speak correctly.

6. That this desire to learn persisted throughout the learning period because for the first time in her life her efforts to learn to speak correctly were being crowned with real success.

7. That her aunt who stayed with her during every period of instruction learned enough about Wilma's difficulties and the methods of overcoming them to give intelligent and effective help between periods of regular instruction at the Clinic.

This report of the work and accomplishments with Wilma is published because there are many other children who are victims not only of detrimental speech habits but of other equally serious habits which will respond equally as satisfactorily to proper instruction if diagnosed and treated early enough. Early correct diagnosis and proper treatment of real causes can do wonders. Failure results from incorrect diagnoses and attempts to treat symptoms as if they were causes. This latter is what would have happened had Wilma been given "strict discipline" for her "bad" conduct, instead of being instructed in correct speech habits. "Bad" conduct in children is always a symptom which can be removed by discovering its underlying causes and treating them. In this case the cause was improper speech habits. In other cases it is malnutrition, or defective vision, or defective hearing, or mistreatment at home or in school, or feeblemindedness, or insanity, or indigestion, or being petted too much, or physical deformity, or any one of a thousand other things. The variety and inter-relation of possible causes is unlimited. The variety and adaptations of methods of treatment must be just as large and unlimited as that of the causes. The particular combination of causes operating in each individual case must determine the method or methods of procedure to be employed. It is easy to understand how in Wilma's case her unintelligible speech was directly the cause of her misconduct. People

could not understand her. She was asked to repeat her statements over and over and then people with an expression which betrayed their attitude toward her pretended to understand. Could a child who had to spend half an hour to make her family understand she meant to say a thing was "yellow," reasonably be expected to be happy, sweet-dispositioned and invariably obedient? Suppose she had been severely "disciplined," instead of instructed to speak correctly, what could have been expected of her as an adolescent and as an adult?

THE INTELLIGENCE OF FULL BLOOD INDIANS

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The American Indian as a subject of study for several years has attracted the attention of students of racial psychology. However, there have been handicaps to satisfactory investigations.

For one thing it has been rather difficult to obtain subjects of pure Indian blood in sufficiently large numbers to make the results very significant. Another difficulty has been in finding for the results so far obtained on these full blood Indians a basis of comparison with white subjects measured by the same tests.

The latter difficulty is largely, though not altogether, due to the same fact, insufficiently large groups. Norms for whites on various standard tests are too low on the age scale to be of much assistance because of the advanced age of the Indians so far tested. That is to say, the Indians are too old to be compared with the whites. Very few full blood Indians of years eight, nine and ten have been tested. They are in fact found to be around twelve, thirteen and fourteen years old when they are obtained in numbers of any magnitude in testing expeditions and we have rather unsatisfactory white norms for these ages. Sometimes the Indian ages run as high as eighteen and nineteen years and occasionally twenty-two and twenty-three years. What is more, an adequate number of the Indians have not, age for age, the educational equivalent of the white subjects. If we take Indians of the fourth grade they will be found to be about fourteen years old on the average and the whites of the same grade will be

around nine years. If we undertake then to control age the factor of education "runs at large" and if we try to control the educational factor the age element in the problem "runs riot." What is needed is some means for controlling the one or the other. In the light of previous practice in general and for the sake of convenience here the practical thing to do in the study of Indian intelligence is to control the age factor if this can be done. This is now made possible by considering, if only tentatively, the mental age equivalents of group intelligence test norms for whites as obtained from large numbers in the United States and determining what the intelligence of an Indian would be under the same circumstances. This, to be sure, disregards the factor of social status which, as between the average white in the United States and the average Indian is certainly different, qualitatively and quantitatively. This is true of Indian children in the United States Indian Schools as well as in their homes on the reservations, but one would be inclined to think it less so in the former case. One has but to visit the Indian in his hogan, his tepee, or pueblo hut or to see him in his community life to be convinced of the meagreness of these as any sort of adequate preparation for the ways and customs of civilization. It has been said to the writer by an Indian school man and reservation administrator of much experience that he would not, if it were in his power, destroy the hogan with its home atmosphere and teaching since from it, he confidently believes, have come some of the "finest characters" he has known. It is true one has to admire the Navajo for his rugged honesty and thriftiness, the Ute for his straightforwardness, the Pueblo for his amiability, and the Sioux and Arapahoe for their courage and endurance. But these, while they may be provided for severally, in the hogan, the adobe hut, the tepee and in the community life, possibly through both heredity and social environment, are not necessarily the traits measured by intelligence tests.

Some of the most commonplace experiences for the average white are lacking to the average Indians. For instance the hogan and tepee have no "chairs" "tables," "cellars," "bureaus"

"teddy-bears," "flower-pots," "cooking stoves," "books," or "clocks." Once in a while we find an iron bed, a trunk, a cooking stove, in a Pueblo home. On one such occasion we marveled at the presence of a kitchen cabinet. We will say nothing of combs, pianos, electric lights, looking glasses and lanterns, such common appointments of the home of the white child. As we approach nearer to the white settlements we find them and that the Indian child in a United States Indian School has some familiarity with such as these is a fact. But a knowledge about such commonplace things and their significance in civilization is presupposed in those who are to pass satisfactorily the white man's intelligence tests.

In the present study we have endeavored to get round all the difficulties mentioned and have been successful in meeting all of them but the last one—that of social status. We have met this as best we could by taking children from a white man's environment as found in the United States Indian School.

With respect to the factor of selection we cannot say that ours is strictly a random sampling. The large number of cases in our study selected from a wide geographical area would indicate a random sampling. Still we have to admit that the Indian boarding school would be in some instances an instrument for selecting the brighter Indians. This would obtain in the school far removed from the Indian home. Nevertheless many schools are located immediately on the reservation and here the factor of selection would not really be selective any more than it is in many white rural communities. The subjects of our study are, we think, fairly representative of their Indian communities, since all the United States Indian Schools are fairly close to the communities from which our subjects come.

In this study we have several objectives: (1) To ascertain the approximate intelligence quotient (IQ) of the full blood Indians in the United States. (2) to see if the IQ differs from grade to grade; (3) to find what correlation exists between this approximate IQ and formal education; (4) to ascertain the median mental age for each school grade of these Indians.

The total number of subjects for whom we have obtained IQ's and mental ages in this study is 1102. Fifty-two of them are from the ninth grade and may not be included legitimately in the large group from which we obtain a tentative norm. We have used these fifty-two ninth graders merely to see if their superior education will favor them above the others in the grades below them. If we deduct the number of the ninth grade group from the total number tested we have a group of one thousand and fifty full blood Indian children of plains and southeastern tribes and those of the southwestern plateau tribes as the Pueblo, Navajo and Apache tribes.[1] The Indian schools visited were the United States Schools located respectively at Chilocco in Oklahoma and Albuquerque, Sante Fe and Shiprock in New Mexico.

The facts about degree of Indian blood and age are not as securely established as one might desire but since such facts are given and accepted by the United States Government we feel that on the whole they are correct. At least we find no other source of information than that of which the government avails itself. The tests used were the National Intelligence Tests, Scale A, Form I.

Table 1 gives for these full blood Indians the distributions of the approximate IQ's of the fourth up through the eighth grade; the sub-totals of these; the totals for the one thousand and fifty; and the IQs also for purposes of comparison of the fifty-two ninth graders. We have given here also the medians and Q's for the grade groups and the larger total group.

An examination of the total distribution in table 1 reveals that we have an approximately normal curve but for the high frequency at the lower end of the scale. This situation may be explained rather satisfactorily by noting that the high spur is due to undistributed IQ's. There were so many Indian children scoring lower than any authoritative mental

[1] Part of the data for the group of one thousand and fifty was obtained on an expedition financed by the Grants Committee of the American Association for the Advancement of Science in the Spring of 1921. The rest of the data was obtained on subsequent expeditions.

age supplied below that of eight and one-half years. This would seem an unusual situation. In the fourth grade there

TABLE 1

Distribution of N. I. T. approximate IQ's for separate grades and total for 1050 full blood Indians

IQ (APPROXIMATE)	GRADES						
	Fourth	Fifth	Sixth	Seventh	Eighth	Total	Ninth
135–139			1			1	
130–134							
125–129	1					1	
120–124	1					1	
115–119							
110–114					2	2	
105–109		2	2			4	
100–104	2	1	2	2	1	8	1
95– 99	1	3	9	3	6	22	2
90– 94	3	9	4	8	4	28	2
85– 89	6	7	14	13	12	52	11
80– 84	18	19	21	30	6	94	12
75– 79	18	20	39	36	9	122	14
70– 74	26	28	59	25	9	147	6
65– 69	23	40	58	29	9	159	4
60– 64	17	32	37	17	4	107	
55– 59	15	28	24	9	0	76	
50– 54	6	16	5	1		28	
(Below or) 0– 49	138	36	22	2		198	
Totals	275	241	297	175	62	1050	52
Median IQ	50.0	66.1	70.2	75.6	80.0	68.6	80.08
Q, semi inter-quartile range	—	9.3	7.1	7.2	8.8	10.3	5.1
Median M.A.	8.5	9.2	10.6	11.6	13.1	—	12.7
Q, semi inter-quartile range	—	1.0	0.9	1.0	1.1	—	0.9

were one hundred and thirty-eight Indians with mental ages below eight and a half years. In the fifth grade there were thirty-six, in the sixth grade twenty-two and in the seventh

grade two. It would be unexampled to find such a situation among white children where half of the children of the fourth grade have a mental age of eight and a half years and less. Calculating a median IQ for such a distribution is beset with some precariousness. We have undertaken it however and find the IQ for the fourth grade to be around 48.5. The median IQ for the fifth grade jumps to 66.1, that for sixth grade is 70.2 and for the seventh and eighth grades it is respectively 75.6 and 80.0. Ninth grade students are not supposed to be tested with the National Intelligence Test. But it is interesting to note that the median IQ for fifty-two ninth grade Indians is 81.8 or about the same as that of the eighth grade group of Indians. It will be seen that the median of the one thousand and fifty IQ's is 68.6 with a Q of 10.3. Calculating

TABLE 2

The per cent of cases attaining and exceeding the median IQ of the preceding grade

	PER CENT
Fifth grade on fourth grade median	83
Sixth grade on fifth grade median	69
Seventh grade on sixth grade median	67
Eighth grade on seventh grade median	64
Ninth grade on eighth grade median	51

averages, average deviations and probable errors directly seemed an unprofitable procedure because of the undistributed IQ's and for that reason we have computed only the medians and the Q's.

It will be seen on examining table 1 again that there is a steady rise in the median IQ in going from grade to grade, and upon availing ourselves of the facts of table 2 we see that the differences from grade to grade tend at first to be marked with the tendency becoming regularly less strong as we proceed from grade to grade. The overlapping reads 83, 69, 67, 64, and 51 per cent respectively as we compare the lower grade IQ's with that of the successively higher grades.

The computation of a coefficient of correlation between IQ

and grade did not appear to be a worthwhile task because of the large number of the undistributed IQ's at the lower end of the scale. Accordingly we have taken the upper four-fifths of the cases, disregarding the undistributed IQ's, and obtained a correlation of 0.25 $\pm$0.021. This would indicate that education while a positive factor in determining the IQ is not as great as one might expect for the upper four-fifths of the cases. It might, however, signify that only the more intelligent full blood Indians feel a disposition to stay at school since the number of cases actually decreases as we go from the sixth grade on to the ninth grade.

To be sure these one thousand fifty Indians are generally retarded educationally but it seems worth while to inquire

TABLE 3

	GRADE					
	High Third	Fourth	Fifth	Sixth	Seventh	Eighth
Indian Median.....	—	8.5	9.2	10.6	11.6	13.1
Q, semi inter-quartile range....	—	—	1.0	0.9	1.0	1.1
White Av. M.A. norm in years..	8.8	9.8	11.2	12.0	13.2	14.3
Difference in years.....	—	1.3	2.0	1.4	1.6	1.2

Average difference 1.5 years between white and Indian children.

what is the corresponding mental age as revealed by the test for each grade and how far below the mental age of the whites they fall. Table 3 gives the median mental age for each grade group for full blood Indians, the averge mental age of the white children for the corresponding grades, which are used as norms, and the differences between these Indian medians and the norms for the whites.

It will be seen that the differences in mental age between white and Indian children vary as follows,—least in the eighth grade, 1.2 years; in the fourth grade, 1.3 years; in the sixth grade 1.4 years; seventh grade 1.6 years, and greatest in the fifth grade, 2 years. On the average the difference in mental age is 1.5 years, disregarding the chronological age of

the Indians. The ratio between mental age of these Indians and of whites of grades 4 to 8 in the United States on the average is 100 to 114 or the whites are 14 per cent better than the Indians. But if on the other hand we take into consideration the chronological age up to sixteen years the ratio of IQ's is about 69 to 100.

Briefly we may conclude as follows:

1. The approximate IQ's of full blood Indians of plains and southwestern tribes in 69.

2. There is a constant tendency for IQ's as found to increase with education.

3. The correlation of distributed IQ's and school grade is a small positive one (0.25) which may signify selection of better Indians with increasing ability to pass the test with education.

4. The mental age of these Indians and whites in the United States grade for grade stands in a ratio of 100 to 114, the whites being 14 per cent better than the Indians.

5. The social status of the Indians in a United States Indian School is more nearly that of the average white than of an Indian not so favored.

6. Because of differences in social status and temperament we cannot conclude that our results are true and final measures of the intelligence of Indian children.

A NEW POINT PERFORMANCE SCALE

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In the course of our work in a child guidance clinic, we meet a large number of children for whom it is highly desirable to obtain a performance scale rating, and a few for whom such a rating is the only obtainable index of general intelligence.

The problem of constructing a performance scale from tests available at the present time is two-fold. First, the most useful tests must be selected, and second, results from the tests selected must be combined in such a way as to yield the most useful values.

In connection with the construction of a group point scale for the measurement of intelligence, the writer had occasion to investigate the relative usefulness of the point method used in the present work, the year method of the Binet scales, and the median mental age method of computing mental age. The degree of correlation with individual test results of mental ages obtained from the same data by the three methods was in the above order, the point method showing the most agreement, the median mental age, the least. Averages of values obtained by the three methods agree closely, but in certain individual cases the median mental age behaves in erratic fashion that gives a mental age rating three or four years below that obtained by either the point method or the individual Binet.

In the selection of tests, the discriminative value formula $\left(\frac{Av_2 - Av_1}{\frac{\sigma_2 + \sigma_1}{2}}\right)$ used and described in "An absolute intelligence scale: A study in method," by Arthur and Woodrow (1) was applied in a modified form. As most of the norms were taken

from Pintner and Paterson (2), the median and Q values were substituted for average and standard deviation. Results from applying this formula to norms available at the present time indicate the Kohs Block Design test has the highest discriminative value, for chronological ages five to fifteen inclusive, of any of the tests considered. Sylvester's norms for the Seguin form board (time) show the next highest discriminative capacity. Porteus has not included a measure of variability in the description of his maze test. On the basis of results from 343 clinic cases, it ranks third in discriminative value. Mare and Foal (time) ranks fourth, and Knox Cube test, fifth. Of the two Healy Picture Completion tests, Picture II exhibits slightly greater discriminative value, but Picture I is more useful for lower age groups, and therefore is included in the present scale. The Manikin (score) and Feature Profile (time score), regarded as two parts of a single test, are fairly useful. Combining time and error scores of the Casuist form board into a single value, increases the practical usefulness of this test. The Two Figure form board, from the standpoint of error scores, appears to be practically useless, but from the standpoint of time scores has a limited value.

The tests included in the present point performance scale therefore, are:

1. Knox Cube
2. Seguin Form Board (time)
3. Two Figure Form Board (time)
4. Casuist Form Board (time and error combined)
5. Manikin (score)
Feature Profile (time)
6. Mare and Foal (time)
7. Healy Picture Completion I (score)
8. Kohs Block Design (score)

In table 1 the discriminative value of each of the above tests, is given for each chronological age level from five to fifteen, inclusive. The maze is not included because of the uncertain value of the clinic data available. These discriminative values furnish the point values of the scale. In the column headed

"Scale" is given the cumulative point value corresponding to each crude score norm. These cumulative point values constitute the point scales of the separate tests. As they are all in terms of the same measure of variability, they may be considered as being made up of comparable units. The point values for the separate tests may, therefore, be totalled for each age level. The result is a combined point scale, as shown in table 2.

TABLE 1

Discriminative value of each test of the scale for chronological age levels 5 to 15, inclusive

Knox Cube Test

C.A.	CASES	SCORE	D.V.	SCALE
5 yr.	61	2		X
6	49	4	1.60	1.60
7	56	5	1.–	2.60
8	57	6	1.33	3.93
9	70	6		
10	95	6		
11	94	7	1.33	5.26
12	101	7		
13	77	7		
14	94	8	0.80	6.06
15	62			

Seguin Form Board

C.A.	CASES	TIME SCORE	D.V.	SCALE
5 yr.	80	37		X
6	170	26	2.20	2.20
7	173	23	1.00	3.20
8	206	20	1.33	4.53
9	214	18	1.00	5.53
10	221	16	1.00	6.53
11	172	15	0.56	7.09
12	141	14	0.67	7.76
13	80	12	1.33	9.09
14	80	11	0.67	9.76
15				

TABLE 1—*Continued*

Two Figure Form Board (Pintner)

C.A.	CASES	TIME SCORE	D.V.	SCALE
5 yr.	54	200		X
6	118	175	0.23	0.23
7	115	116	0.53	0.76
8	142	62	0.71	1.47
9	95	47	0.44	1.91
10	106	47		
11	123	38	0.37	2.28
12	103			
13	55	29	0.63	2.91
14	39			
15	19			

Casuist Form Board (Knox)

C.A.	CASES	TIME			ERRORS		
		Score	D.V.	Scale	Score	D.V.	Scale
5 yr.	29	DNC		X	DNC		X
6	106	300		X	30		X
7	123	154	1.18	1.18	12	1.18	1.18
8	144	106	0.71	1.89	8	0.47	1.65
9	121	93	0.25	2.14	7	0.17	1.82
10	134	78	0.33	2.47	5	0.38	2.20
11	126	68	0.35	2.82	5		
12	56	66	0.08	2.90	4	0.28	2.48
13	44				3	0.44	2.92
14	35	58	0.37	3.27	3		

Manikin (Pintner)

C.A.	CASES	SCORE	D.V.	SCALE
5 yr.	68	3		X
6	115	4	1.–	1.–
7	120	4		
8	127	5	1.33	2.33
9	74	5		
10	25	5		
11	20	5		
12	18	5		
13	8	5		
14	5	5		
15	1	5		

TABLE 1—*Continued*

Feature Profile (Knox and Kempf)

C.A.	CASES	TIME SCORE	D.V.	SCALE
5 yr.				
6	46	DNC		
7	59	DNC		
8	77	DNC		
9	81	DNC		X
10	95	240	1.–	1.–
11	86	157	0.95	1.95
12	82			
13	68	150	0.09	2.04
14	65	132	0.21	2.25
15	34			

Mare and Foal (Healy)

C.A.	CASES	TIME SCORE	D.V.	SCALE
5 yr.	29	107		X
6	67	71	1.18	1.18
7	85	62	0.49	1.67
8	98	48	0.98	2.65
9	72	41	0.74	3.39
10	77	36	0.65	4.04
11	81	34	0.28	4.32
12	70			4.32
13	42	29	0.91	5.23
14	27			
15	16	28	0.20	5.43

Picture Completion Test (Pintner and Anderson)

C.A.	CASES	TIME SCORE	D.V.	SCALE
5 yr.	20	89		X
6	150	153	0.68	0.68
7	150	239	0.88	1.56
8	152	328	0.85	2.41
9	150	407	0.85	3.26
10	150	435	0.32	3.58
11	155	455	0.24	3.82
12	150	493	0.44	4.26
13	150	405	0.15	4.41
14	109	515	0.14	4.55
15	52	525	0.14	4.69

TABLE 1—*Concluded*

Kohs Block Design

C.A.	CASES	SCORE	D.V.	SCALE
5 yr.	1	0		X
6	15	2	3.10	3.10
7	27	6	1.70	4.80
8	28	11	0.90	5.70
9	34	18	1.10	6.80
10	32	25.5	0.70	7.50
11	31	36	0.50	8.00
12	37	48	0.30	8.30
13	33	61	0.30	8.60
14	20	75	0.30	8.90
15	19	89	0.50	9.40

It is obvious that for tests of approximately equal value, the matter of weighting can be safely neglected. It is taken care of by the selection of tests. Among tests of very unequal value, however, the weighting is probably of significance. An inspection of table 2 shows that the tests with the higher discriminative values, such as Kohs Block Design and the Seguin Form Board, are automatically weighted, by the present method of scale construction, so that success on either earns three times as many points as a corresponding degree of success on a test of low discriminative value, such as the Two Figure form board. In the course of time, enough tests of high discriminative value will be available, doubtless, to enable us to discard the poorer tests of the present scale, but in the meantime we need to make the best possible use of what we have at hand. By averaging the point values of the time and error scores of the Casuist form board, we avoid giving double weight to a relatively poor test.

The procedure in giving and scoring the various tests has been that described by Pintner and Paterson, by Kohs for the Block Design, and by Pintner and Anderson for the Picture Completion. It was found, however, that Sylvester's norms, based upon average and superior children, were quite useless for clinic work when his procedure was followed. Goddard's

TABLE 2

Combined Point Scale

C.A.	CUBE TEST		SEGUIN FORM BOARD		MANIKIN AND FEATURE PROFILE		TWO FIGURE FORM BOARD		CASUIST FORM BOARD				AVERAGE	MARE AND FOAL		HEALY PICTURE COMPLETION I		KOHS BLOCK DESIGN		TOTAL
									Errors		Time									
5	2	X	37	X	3	X	200	X	DNC	X	DNC	X		107	X	89	X	0	X	X
6	4	1.60	26	2.20	4	1.0	175	0.23	30	0.84	300	0.44	0.64	71	1.18	153	0.68	2	3.10	10.63
7	5	2.60	23	3.20	4	1.0	116	0.76	12	2.02	154	1.62	1.82	62	1.67	239	1.56	6	4.80	17.41
8	6	3.93	20	4.53	5	2.33	62	1.47	8	2.49	106	2.33	2.41	48	2.65	328	2.41	11	5.70	25.43
9	6	3.93	18	5.53			47	1.91	7	2.66	93	2.58	2.62	41	3.39	407	3.26	18	6.80	29.77
10	6	3.93	16	6.53	240	3.33	47	1.91	5	3.04	78	2.91	2.98	36	4.04	435	3.58	26	7.50	33.80
11	7	5.26	15	7.09	157	4.28	38	2.28	5	3.04	68	3.26	3.15	34	4.32	455	3.82	36	8.00	38.20
12	7	5.26	14	7.76	157	4.28	38	2.28	4	3.32	66	3.34	3.33	32	4.68	493	4.26	48	8.30	40.15
13	7	5.26	12	9.09	150	4.37	29	2.91	3	3.76	66	3.34	3.55	29	5.23	505	4.41	61	8.60	43.42
14	8	6.06	11	9.76	132	4.58	29	2.91	3	3.76	58	3.71	3.74	29	5.23	515	4.55	75	8.90	45.73
15	8	6.06	11	9.76	132	4.58	29	2.91	3	3.76	58	3.71	3.74	28	5.43	525	4.69	89	9.40	46.57

procedure for this test was adopted, with more satisfactory results. The board is placed with the star on the side toward the child, for all three trials. The child sees the blocks removed. The blocks are placed in the same order each time, with the star on the bottom of the pile at the upper right corner of the board (from the child's position), the circle on the star, and the diamond on the circle. For the middle pile the cross is on the bottom, then the lozenge, then the semi-cricle, and the square on top. At the upper left corner of the board is the elipse, the triangle on that, and the oblong on top. For the Healy Picture Completion I, Dr. Florence Goodenough, then of the Minneapolis Child Guidance Clinic, and the writer agreed upon the following fixed order for the arrangement of the blocks:

(Top row): 1, bucket, 2, M. bottle, 3, wheel, 4, blank, 5, pipe, 6, shoe, 7, dog, 8, purse, 9, B. window, 10, fruit, 11, tie.

(Second row from top): 12, cup, 13, knife, 14, chicken, 15, blank, 16, hatchet, 17, blank, 18, basket, 19, blank, 20, pumpkin, 21, blank, 22, S. bird, 23, football, 24, baby.

(Third row from top): 25, baseball, 26, cat, 27, blank, 28, candle, 29, bottle, 30, hat, 31, blank, 32, fish, 33, glove, 34, mouse, 35, sprinkling can, 36, blank.

(Bottom row): 37, flowers, 38, D. cat, 39, blank, 40, F. bird, 41, books, 42, blank, 43, C. window, 44, clock, 45, scissors, 46, stool, 47, cage, 48, S. cat, 49, log, 50, cherries.

The results obtained with the above scale have more than justified expectation. Few of the tests are adequately standardized. Kohs has used only 366 cases in all. In norms for part of the tests, "five years" means 5.0, while for the others it means 5.5, and so on for the other age groups. The separate tests of the scale have been standardized upon different populations. New norms for the various tests must be obtained from a single population before this can be considered a scale in any real sense. As we have seen, norms obtained by Sylvester are being used to evaluate scores obtained with the Goddard form board and mode of procedure. One feels that the tests used must be of great inherent value to yield any worthwhile results under such conditions.

In table 3 are given the values interpolated between the year norms of table 2. These make it possible to convert total point scores into years and months of mental age.

In table 4 are given point values for all possible scores between the five year norm and the highest age norm of each test. The crude score for each test is converted into points by means of this table. These point values are totalled and converted into mental age by reading from table 2. By extrapolation, point values are assigned to scores above the highest year norm of each test. Quotients are then calculated upon a fifteen year

TABLE 3

For converting point scores into years and months of mental age

YEARS	MONTHS											
	0	1	2	3	4	5	6	7	8	9	10	11
5	X	0.88	1.77	2.65	3.54	4.42	5.31	6.19	7.08	7.96	8.85	9.74
6	10.63	11.19	11.76	12.32	12.89	13.45	14.02	14.58	15.15	15.71	16.28	16.84
7	17.41	18.07	18.74	19.41	20.08	20.75	21.42	22.09	22.76	23.43	24.10	24.77
8	25.43	25.79	26.15	26.51	26.87	27.23	27.60	27.96	28.32	28.68	29.04	29.40
9	29.77	30.10	30.44	30.77	31.11	31.44	31.78	32.11	32.45	32.78	33.12	33.45
10	33.80	34.17	34.53	34.90	35.27	35.63	36.00	36.47	36.83	37.20	37.57	37.93
11	38.20	38.36	38.52	38.68	38.85	39.01	39.17	39.33	39.50	39.66	39.82	39.98
12	40.15	40.42	40.69	40.96	41.24	41.51	41.78	42.05	42.33	42.60	42.87	43.14
13	43.42	43.61	43.80	43.99	44.19	44.38	44.57	44.76	44.96	45.16	45.34	45.53
14	45.73	45.80	45.87	45.94	46.01	46.08	46.15	46.22	46.29	46.36	46.43	46.50
15	46.57											

basis. This extends the usefulness of the scale to the higher age groups. The highest score earned by a clinic patient was that of a thirteen-year-old boy. It gave him a quotient of 155. His IQ on the Kuhlmann-Binet Scale was 154.

The principles underlying the method of scale construction here employed are discussed in full in "A group point scale for the measurement of intelligence: A further study in method" (1). For the present, it is enough to state that in spite of the difference in procedure, the principles are basically the same as those of the Binet scale. For this reason it is permissible to compare results from the two scales.

TABLE 4

For determining the point value of any score

CUBE TEST (KNOX) P. & P. NORMS		
3	1	
4	1	−0.80
5 yr.	2	X
	3	0.80
6	4	1.60
7	5	2.60
8	6	3.93
9	6	3.93
10	6	3.93
11	7	4.26
12	7	4.26
13	7	4.26
14	8	5.06
15	8	5.06

+0.80 for each additional line.

SEGUIN FORM BOARD (SYLVESTER)		
5 yr.	37	X
	36	0.20
	35	0.40
	34	0.60
	33	0.80
	32	1.00
	31	1.20
	30	1.40
	29	1.60
	28	1.80
	27	2.00
6 yr.	26	2.20
	25	2.53
	24	2.87
7 yr.	23	3.20
	22	3.64
	21	4.09
8 yr.	20	4.53

SEGUIN FORM BOARD (SYLVESTER)—*Continued*		
	19	5.03
9 yr.	18	5.53
	17	6.03
10 yr.	16	6.53
11 yr.	15	7.09
12 yr.	14	7.76
	13	8.42
13 yr.	12	9.09
	11	9.76

+0.67 for each additional decrease of one second.

TWO FIGURE FORM BOARD (PINTNER)		
5 yr.	200	X
	199	0.01
	198	0.02
	197	0.03
	196	0.04
	195	0.05
	194	0.06
	193	0.07
	192	0.08
	191	0.09
	190	0.09
	189	0.10
	188	0.11
	187	0.12
	186	0.13
	185	0.14
	184	0.15
	183	0.16
	182	0.17
	181	0.18
	180	0.18

TABLE 4—*Continued*

TWO FIGURE FORM BOARD (PINTNER)—*Continued*			TWO FIGURE FORM BOARD (PINTNER)—*Continued*		
	179	0.19		139	0.56
	178	0.20		138	0.57
	177	0.21		137	0.58
	176	0.22		136	0.59
6 yr.	175	0.23		135	0.59
				134	0.60
	174	0.24		133	0.61
	173	0.25		132	0.62
	172	0.26		131	0.63
	171	0.27		130	0.64
	170	0.28		129	0.65
	169	0.29		128	0.66
	168	0.30		127	0.67
	167	0.31		126	0.68
	166	0.32		125	0.68
	165	0.32		124	0.69
	164	0.33		123	0.70
	163	0.34		122	0.71
	162	0.35		121	0.72
	161	0.36		120	0.73
	160	0.37		119	0.74
	159	0.38		118	0.75
	158	0.39		117	0.76
	157	0.40	7 yr.	116	0.76
	156	0.41			
	155	0.41		115	0.77
	154	0.42		114	0.78
	153	0.43		113	0.80
	152	0.44		112	0.81
	151	0.45		111	0.82
	150	0.46		110	0.84
	149	0.47		109	0.85
	148	0.48		108	0.86
	147	0.49		107	0.88
	146	0.50		106	0.89
	145	0.50		105	0.90
	144	0.51		104	0.92
	143	0.52		103	0.93
	142	0.53		102	0.94
	141	0.54		101	0.96
	140	0.55		100	0.97

TABLE 4—*Continued*

TWO FIGURE FORM BOARD (PINTNER)—*Continued*		
	99	0.98
	98	1.00
	97	1.01
	96	1.02
	95	1.04
	94	1.05
	93	1.06
	92	1.08
	91	1.09
	90	1.10
	89	1.12
	88	1.13
	87	1.14
	86	1.16
	85	1.17
	84	1.18
	83	1.20
	82	1.21
	81	1.22
	80	1.24
	79	1.25
	78	1.26
	77	1.28
	76	1.29
	75	1.30
	74	1.32
	73	1.33
	72	1.34
	71	1.36
	70	1.37
	69	1.38
	68	1.40
	67	1.41
	66	1.42
	65	1.44
	64	1.45
	63	1.46
8 yr.	62	1.47
	61	1.50
	60	1.53
	59	1.56
	58	1.59
	57	1.62
	56	1.65
	55	1.68
	54	1.71
	53	1.74
	52	1.77
	51	1.80
	50	1.83
	49	1.86
	48	1.89
9 yr.	47	1.91
	46	1.95
	45	1.99
	44	2.03
	43	2.07
	42	2.11
	41	2.15
	40	2.19
	39	2.23
11 yr.	38	2.28
	37	2.35
	36	2.41
	35	2.48
	34	2.55
	33	2.62
	32	2.69
	31	2.76
	30	2.83
13 yr.	29	2.91
	28	2.98
	27	3.05
	26	3.12
	25	3.19

TWO FIGURE FORM BOARD (PINTNER)—*Continued*

+0.07 for each additional decrease of one second in time.

TABLE 4—*Continued*

CASUIST FORM BOARD (KNOX) TIME		
5 yr.	DNC	X
6 yr.	300	0.44
	299	0.45
	298	0.46
	297	0.47
	296	0.48
	295	0.48
	294	0.49
	293	0.50
	292	0.51
	291	0.52
	290	0.52
	289	0.53
	288	0.54
	287	0.55
	286	0.56
	285	0.56
	284	0.57
	283	0.58
	282	0.59
	281	0.60
	280	0.60
	279	0.61
	278	0.62
	277	0.63
	276	0.64
	275	0.64
	274	0.65
	273	0.66
	272	0.67
	271	0.68
	270	0.68
	269	0.69
	268	0.70
	267	0.71
	266	0.72
	265	0.72
	264	0.73
	263	0.74
	262	0.75

CASUIST FORM BOARD (KNOX) TIME—*Continued*	
261	0.76
260	0.76
259	0.77
258	0.78
257	0.79
256	0.80
255	0.80
254	0.81
253	0.82
252	0.83
251	0.84
250	0.84
249	0.85
248	0.86
247	0.87
246	0.88
245	0.88
244	0.89
243	0.90
242	0.91
241	0.92
240	0.92
239	0.93
238	0.94
237	0.95
236	0.96
235	0.96
234	0.97
233	0.98
232	0.99
231	1.00
230	1.00
229	1.01
228	1.02
227	1.03
226	1.04
225	1.04
224	1.05
223	1.06
222	1.07

TABLE 4—*Continued*

CASUIST FORM BOARD (KNOX) TIME—*Continued*		CASUIST FORM BOARD (KNOX) TIME—*Continued*	
221	1.08	181	1.40
220	1.08	180	1.40
219	1.09	179	1.41
218	1.10	178	1.42
217	1.11	177	1.43
216	1.12	176	1.44
215	1.12	175	1.44
214	1.13	174	1.45
213	1.14	173	1.46
212	1.15	172	1.47
211	1.16		
210	1.16	171	1.48
209	1.17	170	1.48
208	1.18	169	1.49
207	1.19	168	1.50
206	1.20	167	1.51
205	1.20	166	1.52
204	1.21	165	1.52
203	1.22	164	1.53
202	1.23	163	1.54
201	1.24	162	1.55
200	1.24	161	1.56
199	1.25	160	1.56
198	1.26	159	1.57
197	1.27	158	1.58
196	1.28	157	1.59
195	1.28	156	1.60
194	1.29	155	1.61
193	1.30	7 yr. 154	1.62
192	1.31		
191	1.32	153	1.63
190	1.32	152	1.64
189	1.33	151	1.66
188	1.34	150	1.68
187	1.35	149	1.69
186	1.36	148	1.70
185	1.36	147	1.71
184	1.37	146	1.72
183	1.38	145	1.74
182	1.39	144	1.76

TABLE 4—*Continued*

CASUIST FORM BOARD (KNOX) TIME—*Continued*			CASUIST FORM BOARD (KNOX) TIME—*Continued*		
	143	1.77		103	2.39
	142	1.78		102	2.41
	141	1.79		101	2.43
	140	1.81		100	2.45
	139	1.83		99	2.47
	138	1.84		98	2.49
	137	1.85		97	2.51
	136	1.86		96	2.52
	135	1.88		95	2.54
	134	1.90		94	2.56
	133	1.91	9 yr.	93	2.58
	132	1.92			
	131	1.93		92	2.60
	130	1.95		91	2.62
	129	1.97		90	2.64
	128	1.98		89	2.66
	127	1.99		88	2.69
	126	2.00		87	2.71
	125	2.02		86	2.73
	124	2.04		85	2.75
	123	2.05		84	2.77
	122	2.06		83	2.80
	121	2.07		82	2.82
	120	2.09		81	2.84
	119	2.11		80	2.86
	118	2.12		79	2.89
	117	2.13	10 yr.	78	2.91
	116	2.14			
	115	2.16		77	2.94
	114	2.18		76	2.98
	113	2.19		75	3.01
	112	2.20		74	3.05
	111	2.21		73	3.08
	110	2.23		72	3.12
	109	2.26		71	3.15
	108	2.29		70	3.19
	107	2.31		69	3.22
8 yr.	106	2.33	11 yr.	68	3.26
	105	2.35		67	3.30
	104	2.37	12 yr.	66	3.34

TABLE 4—*Continued*

CASUIST FORM BOARD (KNOX) TIME—*Continued*		
	65	3.38
	64	3.43
	63	3.47
	62	3.52
	61	3.56
	60	3.61
	59	3.66
14 yr.	58	3.71

+0.046 for each additional decrease of one second.

CASUIST FORM BOARD (ERRORS)		
5 yr.	DNC	X
6 yr.	30	0.84
	29	0.90
	28	0.97
	27	1.03
	26	1.10
	25	1.16
	24	1.23
	23	1.29
	22	1.36
	21	1.42
	20	1.49
	19	1.55
	18	1.62
	17	1.68
	16	1.75
	15	1.81
	14	1.88
	13	1.95
7 yr.	12	2.02
	11	2.14
	10	2.26
	9	2.38
8 yr.	8	2.49

CASUIST FORM BOARD (ERRORS)—*Continued*		
9 yr.	7	2.66
	6	2.85
10 yr.	5	3.04
12 yr.	4	3.32
13 yr.	3	3.76
	2	4.20
	1	4.64
MANIKIN		
3 yr.	0	−1.50
	1	−1.00
4 yr.	2	−0.50
5 yr.	3	X
6 yr.	4	1.−
8 yr.	5	2.33
FEATURE PROFILE		
9 yr.	DNC	−0.01
10 yr.	240	3.33
	239	3.34
	238	3.35
	237	3.36
	236	3.37
	235	3.39
	234	3.40
	233	3.41
	232	3.42
	231	3.44
	230	3.45
	229	3.46
	228	3.47
	227	3.48
	226	3.49
	225	3.51

TABLE 4—*Continued*

FEATURE PROFILE—*Continued*		FEATURE PROFILE—*Continued*		
	224	3.52	183	3.98
	223	3.53	182	3.99
	222	3.54	181	4.01
	221	3.56	180	4.02
	220	3.57	179	4.03
	219	3.58	178	4.04
	218	3.59	177	4.05
	217	3.60	176	4.06
	216	3.61	175	4.07
	215	3.63	174	4.08
	214	3.64	173	4.09
	213	3.65	172	4.10
	212	3.66	171	4.11
	211	3.68	170	4.12
	210	3.69	169	4.14
	209	3.70	168	4.15
	208	3.71	167	4.16
	207	3.72	166	4.17
	206	3.73	165	4.18
	205	3.74	164	4.19
	204	3.75	163	4.20
	203	3.76	162	4.21
	202	3.77	161	4.22
	201	3.79	160	4.24
	200	3.80	159	4.25
	199	3.81	158	4.26
	198	3.82	11 yr. 157	4.28
	197	3.83		
	196	3.84	156	4.29
	195	3.85	155	4.30
	194	3.86	154	4.32
	193	3.87	153	4.33
	192	3.88	152	4.34
	191	3.90	151	4.36
	190	3.91	13 yr. 150	4.37
	189	3.92		
	188	3.93	149	4.38
	187	3.94	148	4.39
	186	3.95	147	4.40
	185	3.96	146	4.41
	184	3.97	145	4.42

TABLE 4—*Continued*

FEATURE PROFILE—*Continued*		
	144	4.44
	143	4.45
	142	4.46
	141	4.47
	140	4.48
	139	4.49
	138	4.51
	137	4.52
	136	4.53
	135	4.54
	134	4.55
	133	4.56
14 yr.	132	4.58
	131	
	130	

+0.01⅛ for each additional decrease of one second.

MARE AND FOAL (HEALY)		
5 yr.	107	X
	106	0.03
	105	0.06
	104	0.10
	103	0.13
	102	0.16
	101	0.20
	100	0.23
	99	0.26
	98	0.29
	97	0.32
	96	0.35
	95	0.39
	94	0.42
	93	0.45
	92	0.49
	91	0.52
	90	0.55
	89	0.59

MARE AND FOAL (HEALY)—*Continued*		
	88	0.62
	87	0.65
	86	0.69
	85	0.72
	84	0.75
	83	0.79
	82	0.82
	81	0.85
	80	0.89
	79	0.92
	78	0.95
	77	0.99
	76	1.02
	75	1.05
	74	1.09
	73	1.12
	72	1.15
6 yr.	71	1.18
	70	1.23
	69	1.29
	68	1.34
	67	1.40
	66	1.45
	65	1.51
	64	1.55
	63	1.61
7 yr.	62	1.67
	61	1.74
	60	1.81
	59	1.87
	58	1.94
	57	2.01
	56	2.08
	55	2.15
	54	2.22
	53	2.29
	52	2.36
	51	2.43
	50	2.50
	49	2.57

TABLE 4—*Continued*

MARE AND FOAL (HEALY)—*Continued*			HEALY PICTURE COMPLETION I		
8 yr.	48	2.65	5 yr.	89	X
	47	2.76		90	0.01
	46	2.86		91	0.02
	45	2.97		92	0.03
	44	3.07		93	0.04
	43	3.18		94	0.05
	42	3.28		95	0.06
9 yr.	41	3.39		96	0.07
				97	0.08
	40	3.52		98	0.09
	39	3.65		99	0.11
	38	3.78		100	0.12
	37	3.91		101	0.13
10 yr.	36	4.04		102	0.14
				103	0.15
	35	4.18		104	0.16
11 yr.	34	4.32		105	0.17
				106	0.18
	33	4.50		107	0.19
	32	4.68		108	0.21
	31	4.86		109	0.22
	30	5.04		110	0.23
13 yr.	29	5.23		111	0.24
				112	0.25
15 yr.	28	5.43		113	0.26
				114	0.27
	27	5.63		115	0.28
	26	5.83		116	0.29
	25	6.03		117	0.30
	24	6.23		118	0.31
	23	6.43		119	0.32
	22	6.63		120	0.33
	21	6.83		121	0.34
	20	7.03		122	0.35
	19	7.23		123	0.36
	18	7.43		124	0.37
	17	7.63		125	0.38
				126	0.39
+0.20 for each additional decrease of one second.				127	0.40
				128	0.41

TABLE 4—*Continued*

HEALY PICTURE COMPLETION I—*Continued*			HEALY PICTURE COMPLETION I—*Continued*	
	129	0.42	169	0.84
	130	0.43	170	0.85
	131	0.44	171	0.86
	132	0.45	172	0.87
	133	0.46	173	0.88
	134	0.47	174	0.89
	135	0.48	175	0.90
	136	0.49	176	0.91
	137	0.51	177	0.92
	138	0.52	178	0.93
	139	0.53	179	0.94
	140	0.54	180	0.95
	141	0.55	181	0.96
	142	0.56	182	0.97
	143	0.57	183	0.98
	144	0.58	184	0.99
	145	0.59	185	1.00
	146	0.61	186	1.01
	147	0.62	187	1.02
	148	0.63	188	1.03
	149	0.64	189	1.04
	150	0.65	190	1.05
	151	0.66	191	1.06
	152	0.67	192	1.07
6 yr.	153	0.68	193	1.08
			194	1.09
	154	0.69	195	1.10
	155	0.70	196	1.11
	156	0.71	197	1.12
	157	0.72	198	1.13
	158	0.73	199	1.14
	159	0.74	200	1.16
	160	0.75	201	1.17
	161	0.76	202	1.18
	162	0.77	203	1.19
	163	0.78	204	1.20
	164	0.79	205	1.21
	165	0.80	206	1.22
	166	0.81	207	1.23
	167	0.82	208	1.24
	168	0.83	209	1.25

TABLE 4—*Continued*

HEALY PICTURE COMPLETION I—*Continued*			HEALY PICTURE COMPLETION I—*Continued*	
	210	1.26	250	1.67
	211	1.27	251	1.68
	212	1.28	252	1.69
	213	1.29	253	1.70
	214	1.30	254	1.71
	215	1.31	255	1.72
	216	1.32	256	1.73
	217	1.33	257	1.74
	218	1.34	258	1.75
	219	1.35	259	1.76
	220	1.36	260	1.77
	221	1.37	261	1.77
	222	1.38	262	1.78
	223	1.39	263	1.79
	224	1.40	264	1.80
	225	1.41	265	1.81
	226	1.42	266	1.82
	227	1.43	267	1.83
	228	1.44	268	1.84
	229	1.45	269	1.85
	230	1.46	270	1.86
	231	1.47	271	1.87
	232	1.48	272	1.88
	233	1.49	273	1.89
	234	1.50	274	1.90
	235	1.51	275	1.91
	236	1.52	276	1.92
	237	1.53	277	1.93
	238	1.54	278	1.94
7 yr.	239	1.56	279	1.95
			280	1.96
	240	1.57	281	1.96
	241	1.58	282	1.97
	242	1.59	283	1.98
	243	1.60	284	1.99
	244	1.61	285	2.00
	245	1.62	286	2.01
	246	1.63	287	2.02
	247	1.64	288	2.03
	248	1.65	289	2.04
	249	1.66	290	2.05

TABLE 4—*Continued*

HEALY PICTURE COMPLETION I—*Continued*			HEALY PICTURE COMPLETION I—*Continued*		
	291	2.06		331	2.44
	292	2.07		332	2.45
	293	2.08		333	2.46
	294	2.09		334	2.47
	295	2.10		335	2.48
	296	2.11		336	2.49
	297	2.12		337	2.50
	298	2.13		338	2.51
	299	2.14		339	2.52
	300	2.15		340	2.53
	301	2.15		341	2.55
	302	2.16		342	2.56
	303	2.17		343	2.57
	304	2.18		344	2.58
	305	2.19		345	2.59
	306	2.20		346	2.60
	307	2.21		347	2.61
	308	2.22		348	2.62
	309	2.23		349	2.63
	310	2.24		350	2.64
	311	2.25		351	2.65
	312	2.26		352	2.66
	313	2.27		353	2.67
	314	2.28		354	2.69
	315	2.29		355	t 2.70
	316	2.30		356	2.71
	317	2.31		357	2.72
	318	2.32		358	2.73
	319	2.33		359	2.74
	320	2.34		360	2.75
	321	2.35		361	2.76
	322	2.36		362	2.77
	323	2.37		363	2.78
	324	2.38		364	2.79
	325	2.39		365	2.80
	326	2.40		366	2.81
	327	2.41		367	2.83
8 yr.	328	2.41		368	2.84
				369	2.85
	329	2.42		370	2.86
	330	2.43		371	2.87

TABLE 4—*Continued*

HEALY PICTURE COMPLETION I—*Continued*			HEALY PICTURE COMPLETION I—*Continued*		
	372	2.88		412	3.31
	373	2.89		413	3.32
	374	2.90		414	3.34
	375	2.91		415	3.35
	376	2.92		416	3.36
	377	2.93		417	3.37
	378	2.94		418	3.38
	379	2.95		419	3.39
	380	2.97		420	3.40
	381	2.98		421	3.42
	382	2.99		422	3.43
	383	3.00		423	3.44
	384	3.01		424	3.45
	385	3.02		425	3.46
	386	3.03		426	3.47
	387	3.04		427	3.48
	388	3.05		428	3.50
	389	3.06		429	3.51
	390	3.07		430	3.52
	391	3.08		431	3.53
	392	3.09		432	3.54
	393	3.11		433	3.55
	394	3.12		434	3.56
	395	3.13	10 yr.	435	3.58
	396	3.14			
	397	3.15		436	3.59
	398	3.16		437	3.60
	399	3.17		438	3.61
	400	3.18		439	3.62
	401	3.19		440	3.64
	402	3.20		441	3.65
	403	3.21		442	3.66
	404	3.22		443	3.67
	405	3.23		444	3.68
	406	3.25		445	3.70
9 yr.	407	3.26		446	3.71
				447	3.72
	408	3.27		448	3.73
	409	3.28		449	3.74
	410	3.29		450	3.76
	411	3.30		451	3.77

TABLE 4—*Continued*

HEALY PICTURE COMPLETION I—*Continued*		
	452	3.78
	453	3.79
	454	3.80
11 yr.	455	3.82
	456	3.83
	457	3.84
	458	3.85
	459	3.86
	460	3.87
	461	3.89
	462	3.90
	463	3.91
	464	3.92
	465	3.93
	466	3.94
	467	3.96
	468	3.97
	469	3.98
	470	3.99
	471	4.00
	472	4.01
	473	4.03
	474	4.04
	475	4.05
	476	4.06
	477	4.07
	478	4.08
	479	4.10
	480	4.11
	481	4.12
	482	4.13
	483	4.14
	484	4.15
	485	4.17
	486	4.18
	487	4.19
	488	4.20
	489	4.21
	490	4.22
	491	4.23

HEALY PICTURE COMPLETION I—*Continued*		
	491	4.23
	492	4.24
12 yr.	493	4.26
	494	4.27
	495	4.28
	496	4.29
	497	4.31
	498	4.32
	499	4.33
	500	4.34
	501	4.36
	502	4.37
	503	4.38
	504	4.39
13 yr.	505	4.41
	506	4.42
	507	4.43
	508	4.45
	509	4.46
	510	4.48
	511	4.49
	512	4.50
	513	4.52
	514	4.53
14 yr.	515	4.55
	516	4.56
	517	4.57
	518	4.59
	519	4.60
	520	4.62
	521	4.63
	522	4.64
	523	4.66
	524	4.67
15 yr.	525	4.69

+0.014 for each additional crude score unit.

TABLE 4—*Continued*

KOHS BLOCK DESIGN		
5 yr.	0	X
	1	1.55
6 yr.	2	3.10
	3	3.53
	4	3.95
	5	4.38
7 yr.	6	4.80
	7	4.98
	8	5.16
	9	5.34
	10	5.52
8 yr.	11	5.70
	12	5.86
	13	6.01
	14	6.17
	15	6.33
	16	6.49
	17	6.64
9 yr.	18	6.80
	19	6.90
	20	7.00
	21	7.10
	22	7.20
	23	7.30
	24	7.40
10 yr.	25	7.50
	26	7.54
	27	7.59
	28	7.63
	29	7.68
	30	7.72
	31	7.77
	32	7.81
	33	7.86
	34	7.90
	35	7.95

KOHS BLOCK DESIGN—*Continued*		
11 yr.	36	8.00
	37	8.02
	38	8.05
	39	8.07
	40	8.10
	41	8.12
	42	8.15
	43	8.17
	44	8.20
	45	8.22
	46	8.25
	47	8.27
12 yr.	48	8.30
	49	8.32
	50	8.34
	51	8.37
	52	8.39
	53	8.41
	54	8.44
	55	8.46
	56	8.48
	57	8.51
	58	8.53
	59	8.55
	60	8.58
13 yr.	61	8.60
	62	8.62
	63	8.64
	64	8.66
	65	8.68
	66	8.70
	67	8.72
	68	8.74
	69	8.76
	70	8.78
	71	8.81
	72	8.83
	73	8.85

TABLE 4—*Continued*

KOHS BLOCK DESIGN—*Continued*		
	74	8.87
14 yr.	75	8.90
	76	8.94
	77	8.98
	78	9.02
	79	9.06
	80	9.10
	81	9.14
	82	9.18
	83	9.22
	84	9.25
	85	9.28
	86	9.31
	87	9.34
	88	9.37
15 yr.	89	9.40
	90	9.47
	91	9.55
	92	9.63
	93	9.70
	94	9.78
	95	9.86
	96	9.93
	97	10.01
	98	10.09
	99	10.16
	100	10.24
	101	10.32
16 yr.	102	10.40

KOHS BLOCK DESIGN—*Continued*		
	103	10.56
	104	10.72
	105	10.88
	106	11.04
	107	11.20
	108	11.36
	109	11.52
	110	11.68
	111	11.84
17 yr.	112	12.00
	113	12.13
	114	12.26
	115	12.40
	116	12.53
	117	12.66
	118	12.80
	119	12.93
	120	13.06
18 yr.	121	13.20
	122	13.50
	123	13.80
	124	14.1
	125	14.4
	126	14.7
19 yr.	127	15.00
+0.3 for each additional point.		

For 46 clinic subjects, IQ's are available for both the Kuhlmann-Binet and new point performance scales. To find a measure that would be stable whether the regression was linear or not, and whatever the range of values considered, the probable error of measurement was calculated by Nygaard's (5) formula based on averages. It was found to be 4.39. This is much larger than that obtained between 275 Kuhlmann-Binet IQ's and those of the group point scale above referred to, which was 2.60, but is surprisingly good considering that the P.E.

of the Stanford-Binet scale, as published by Terman (6), is 4.50.

It would seem, then, that a performance scale, when constructed according to Binet principles, can be trusted to do the work of a Binet scale in measuring general intelligence. In its present tentative state of standardization, one would never use it in place of a Binet measure except form necessity. For the deaf child, however, and for the child with speech difficulty, whether the difficulty is one of emotional repression, foreign language handicap or so-called speech defect, it is highly useful in furnishing an intelligence rating that is far more independent of training than are any of the written tests.

The flexibility of the scale adds much to its practical value for clinic use. Separate tests are not infrequently omitted for one reason or another, in giving the scale. In such cases the point value of the omitted test is subtracted from the combined scale norms between which the obtained point score would fall, and the distance above the lower norm is converted into a fraction of a year.

Plans are under way for adequate standardization of the present scale as a whole. The Porteus Maze test will be included, and should increase the value of the scale. It is possible that both form boards will be omitted. A better test in place of the Manikin—Feature Profile would be highly desirable. It is believed that the reliability of the Knox Cube test will be increased by giving it both at the beginning and at the end of the test series.

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A NOTE ON MOTOR ACTIVITY AS CONDITIONED BY EMOTIONAL STATES[1]

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There is scriptural as well as popular warrant for the assertion that there is a positive relationship between pleasurable emotional states and motor activity. To "jump for joy" and to "dance and make a joyful noise before the Lord" obviously are, from a psychological viewpoint, similar if not identical phenomena. Aside from these two weighty authorities it is of practical as well as theoretical importance to measure quantitatively this relationship to ascertain whether science agrees or disagrees with the dicta of Demos and the Bible. To answer the question with reference to a relatively controlled situation was the purpose of the experiment here described.

PROCEDURE

The data were gathered from sophomore engineering students in a beginning course in psychology. The method followed in most essentials the procedure outlined by Langfeld and Allport.[2] The treatment of the quantitative results and the empirical checks described below are the only features of this study for which any originality is claimed. The directions given to students will make clear the procedure.

[1] Responsibility for this study is divided as follows: Thompson and Remmers together collected the data of the main experiment. These data were summarized by Remmers. Thompson suggested the empirical check (see context) and gathered and summarized the data for this. For the statistical treatment and interpretation of all the data and the writing of this article Remmers is wholly responsible.

[2] Langfeld, H. S., and Allport, F. H., An Elementary Course in Laboratory Psychology, p. 146 ff.

DIRECTIONS

You are to think of a very pleasant experience that you have had. When you have reinstated the emotional state as nearly as possible, you will draw free-hand a line of from 2 to 5 inches in length approximately at the middle of the unruled white sheet I have given you. This is to avoid the possibility of your having an objective criterion of judging successive lengths of lines to be drawn later. When you have drawn the line near the top of the middle of the sheet, fold the sheet over in such a manner that you do not see the first line you drew. Now think of a very unpleasant experience. While thinking of this try to draw another line equal in length to the one covered up. Now think of another unpleasant experience. Cover the second line in the same way as the first, attempt again to draw a line of the same length. I have placed on the board a scheme of "double fatigue" procedure in which P = pleasant and U = unpleasant. Note that it goes like this:

P
U
U
P
P
U
etc.

You are to continue this for a total of forty lines. Call the successive pairs A_1, pleasant; A_2, unpleasant; B_2, unpleasant; B_1, pleasant; etc. Use four or five different emotional experiences of each type if you can think of that many.

Before beginning the experiment the instructions were again carefully gone over and an opportunity given for asking questions on any part of the procedure not perfectly clear. When it was quite certain that all understood, the experiment proceeded.

The successive pairs of lines were then measured in millimeters, tabulated, and summarized.

Limitations of space forbid the inclusion of the totals of individual subjects. Suffice it to say they varied for the various categories at the head of the columns as noted in table 1.

As is obvious even from this table, individual variations are large and may go in either direction under the conditions of the experiment. In other words, for some subjects pleasant vs. unpleasant moods produce a greater total length of lines

while for others the results are reversed. The query immediately presents itself: are these fluctuations not a matter of "chance?"[3] The answer in terms of all the data gathered is negative. Table 2 gives the essential facts.

The observed difference of 4329 mm. between the two totals is greater than chance would account for. We may reasonably assume that the two distributions from which these totals are computed are amenable to the criterion of the Bernoulli

TABLE 1

Totals of Individual subjects

	LENGTH OF PLEASANT LINES	LENGTH OF UNPLEASANT LINES	AMOUNT PLEASANT LINES LONGER	PER CENT PLEASANT LINES LONGER	AMOUNT UNPLEASANT LINES LONGER	PER CENT UNPLEASANT LINES LONGER
Maximum.....	4,285	4,220	360	453.4	722	162.4
Minimum.....	511	536	6	4.7	0	0

theorem. The probable error of such a dichotomous division as our data involve is given by the formula.[4]

$$\text{P.E.} = 0.6745\sqrt{\frac{p\,q}{n}}$$

where P.E. = probable error
p = probability of success (i.e., pleasant longer)
q = probability of failure (i.e., unpleasant longer)
$p + q = 1$
n = number of cases

Substituting and solving this equation for the values of pleasant and unpleasant length of lines we obtain

$$\text{P.E.} = 0.6745\sqrt{\frac{(0.509)\ (0.491)}{276167}}$$

$$= 0.00064$$

[3] *Chance* is defined for purposes of this discussion as *ignorance of cause*—the meaning it must be given in a universe viewed from the standpoint of science.

[4] See Rietz, H. L. et al., A Handbook of Mathematical Statistics, p. 74.

This may be interpreted by stating that it is an even wager that the true value of p in terms of percentage is 0.509 ± 0.00064 and that this P.E. indicates that the observed difference could never occur by chance. The odds in favor of this difference being significant runs into billions.[5]

TABLE 2

Totals and averages for all subjects. N = 84

	LENGTH OF PLEASANT LINES	LENGTH OF UNPLEASANT LINES	AMOUNT PLEASANT LINES LONGER	PER CENT PLEASANT LINES LONGER	AMOUNT UNPLEASANT LINES LONGER	PER CENT UNPLEASANT LINES LONGER
	mm.	mm.				
	140,248	135,919	7,895	9,134.2	3,566	4,129.9
Average per subject..	1,669.62	1,618.08	94.0	108.70	42.46	49.17
Average per line.....	83.481	80.904	4.70	5.435	2.123	2.459

TABLE 3

Totals and averages for 51 subjects. Chance lines

LENGTH OF LINE A	LENGTH OF LINE B	LONGER A	PER CENT LONGER A	LONGER B	PER CENT LONGER B
81,866	81,038	2,299	2,733.04	1,755	2,130.67
80.25	79.45	2.15	2.67	1.72	2.25

AN EMPIRICAL CHECK EXPERIMENT

A further sampling of lengths of lines under identical conditions except for the variables of pleasant and unpleasant moods was suggested by Mr. Thompson and was undertaken by him. He used as subjects 51 of the original 84 subjects. They were told to draw forty lines as before, but with no other conditions. Both halves of these were measured in the same order as in the other experiment and were labelled Lines A_1 and B_2 respectively. The results are given in table 3. Applying our criterion of P.E. we obtain

$$\text{P.E.} = 0.6745 \sqrt{\frac{(0\ 50025)\ (0.49975)}{162904}} = 0.00083$$

[5] *Ibid.*, p. 100.

That is, the difference between p and q is less than 1 P.E. and therefore a true chance difference. The assumptions made, therefore, are corroborated by the empirical check.

A SECOND CHECK

Still another check was undertaken[6] by making a frequency distribution of a sample of forty-three individual results. The data are shown in table 4.

TABLE 4

Frequency distribution of pleasant and unpleasant lines. $N = 43$

LENGTH OF LINES	FREQUENCIES	
	Pleasant	Unpleasant
mm.		
25–34		11
35–44	18	22
45–54	84	103
55–64	130	123
65–74	104	108
75–84	123	93
85–94	76	101
95–104	106	93
105–114	62	70
115–124	59	57
125–134	29	45
135–144	29	18
145–154	19	12
155–164	14	4
165–174	1	0
Totals	860	860
Mean	86.78	83.47
σ	28.5	27.7
σ_M	0.972	0.945
Diff.$_M$	3.31 ± 0.914	

It is apparent from the P.E. of the difference of the means that although not as favorable to our conclusions as the appli-

[6] For the statistical labor of this I am indebted to my wife.—H. H. R.

cation of the Bernoulli theorem, the conclusion as to the significance of the difference is still practical certainty.[7]

SOME PRACTICAL AND THEORETICAL CONSIDERATIONS

One of the questions that comes to mind is whether sound pedagogy sanctions the inclusion of this experiment in an elementary manual of laboratory exercises in psychology.[8] The students are asked to interpret results which are likely to disagree as between any two individuals, not only as to relative differences of length of lines but also as to which kind of lines, pleasant or unpleasant, shall be longer. They are asked to do this interpretation, moreover, when they have not become acquainted with statistical techniques adequate to the job. The net result is likely to be a contempt on the part of the undiscriminating student for the subject matter of psychology as a whole, on the basis of the well known mechanism of "associative shifting," to use Thorndike's terminology. They are likely to "throw out the child with the bath."

Another question may be raised as to the causes for the observed individual differences. May the data be considered as supporting any of the various "type" theories of emotion, whether of Doris Blake, Mr. McCutcheon's "Glads" and "Blues" or the ancient classification of temperaments into sanguine-choleric and melancholic-phlemgatic? The answer is no. The variations in results, of which a tabulation might be made, show that there are all degrees of variation from pure chance differences, i.e., no differences, to the variations shown in table 1.

A partial explanation at least is to be found, I believe, in the

[7] For a definition of "practical certainty," see Herring, Jour. Educ. Psych., December, 1924.

[8] Criticism of this particular experiment is by no means to be extended to the rest of Langfeld and Allport's Manual. I have used much of their material with excellent results. In my judgment the value of the Manual would be increased by the inclusion of at least tentative norms of experimental results.

vague meanings attached to "pleasant" and "unpleasant." Several of the subjects volunteered the information that the unpleasant experiences in their case were anger-evoking experiences and these produced longer lines. Introspective analysis and more carefully defined categories of emotion would probably be illuminating in this connection.

SOCIAL STATUS OF THE CLERICAL WORKER AND HIS PERMANENCE ON THE JOB

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During the year 1922, 59 clerical workers were hired in a life insurance company of medium size. Each of the 59 filled out an application blank and on it stated among other things, the occupation of the father.

These parental occupations have been studied and grouped into seven classes:

I. Unskilled laborer
II. Semi-skilled mechanical worker
III. Skilled mechanical worker
IV. Clerical worker
V. Salesman or manager of a department in a company
VI. Proprieter of a small business
VII. Professional man (this group would include managers or owners of large business concerns but there were none in the 59 cases studied)

The classes are given above in the order which, we believe, the average clerk would consider that of their social status. The order of grouping is not that of financial returns. The salary of the skilled mechanical worker and even of the semi-skilled would in many cases exceed that of the clerical worker; however, anyone who has known a large number of clerical workers, especially those employed in an office that is connected with a factory, will, we believe, agree that women, particularly, consider a clerical job to carry a higher social standing than a mechanical job.

Two occupations only, out of the whole number of parental occupations reported, offered difficulty of classification. The first was a policeman, the second, a customs inspector. On

account of the salary received by the policeman, we placed him with the skilled mechanic group. The customs inspector was dropped from the list since we were unable to determine his specific duties or salary.

Figure 1 shows the number of clerks whose fathers fall into each of these occupational groups. It will be noted that the largest number are found in the group of skilled workmen and that, considering the small number of cases involved, a fairly

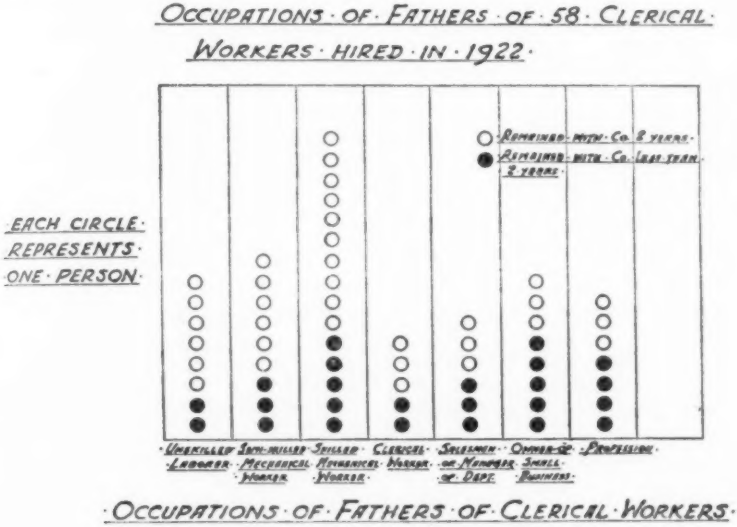
OCCUPATIONS OF FATHERS OF 58 CLERICAL WORKERS HIRED IN 1922
EACH CIRCLE REPRESENTS ONE PERSON
REMAINED WITH CO. 2 YEARS
REMAINED WITH CO. LESS THAN 2 YEARS
UNSKILLED LABORER
SEMI-SKILLED MECHANICAL WORKER
SKILLED MECHANICAL WORKER
CLERICAL WORKER
SALESMEN OR MEMBERS OF DEPT.
OWNER OF SMALL BUSINESS
PROFESSIONS
OCCUPATIONS OF FATHERS OF CLERICAL WORKERS

FIG. 1

regular frequency distribution curve is formed, around that class as a mode.

At the end of 1924 an examination was made to see how many of those entering in 1922 had remained with the company through their second anniversary. It was found that 24 of the 58 employed had left the company, 34 still remaining. That is, there had been in the two years a 41 per cent turnover, or an average turnover of 20.5 per cent each year, a record which all personnel managers would consider very good.

The selection of these 58 workers had been very carefully made on the basis of intelligence and general ability, tests having been given for both general intelligence and trade proficiency. Practically the entire turnover had been voluntary on the part of the employees.

The distribution of this turnover, by the occupations of the fathers, shows an interesting result. This is also shown in figure 1. We have indicated those remaining with the company by an open circle and those leaving the company by a filled in circle. Table 1 gives the same information.

TABLE 1

OCCUPATION OF FATHER	NUMBER OF CLERKS REMAINING FOR TWO YEARS OR MORE	NUMBER OF CLERKS REMAINING LESS THAN TWO YEARS	PER CENT OF CLASS REMAINING LESS THAN TWO YEARS
			per cent
I. Unskilled worker	6	2	25
II. Semi-skilled mechanical worker	6	3	33
III. Skilled mechanical worker	10	5	33
IV. Clerical worker	3	2	40
V. Salesman and department manager	3	3	50
VI. Proprietor of small business	3	5	62
VII. Profession	3	4	57
Total	34	24	41

It is noteworthy that considering the small number of cases involved there is a remarkably consistent increase in the per cent leaving, from class I whose fathers were unskilled laborers to classes VI and VII whose fathers were professional men or proprietors of a small business. The turnover here ranges from 25 to 62 per cent.

A similar study of the employment records of two other companies, under less strict conditions, however, shows the same facts concerning the variation in the length of service of clerical workers from different social strata.

The statistical data are, of course, exceedingly meager, but nevertheless are suggestive. An explanation of the tendency readily suggests itself. If the clerical worker is bettering himself socially by taking a clerical job and continuing in it, he tends to remain with the company, but if the clerical job means a demotion in his social status, he tends to leave the job.

Another explanation also suggests itself. A boy entering the clerical field from the family of an unskilled laborer feels, as he looks forward to possible promotion some day to section or division head at $3000 a year, that the future is very promising. On the other hand, a boy from the family of a salesman or a manager of a small business, does not look upon such a prospect as being very bright. It may be that neither one of these "socially" higher occupations actually yields more than $3000 a year, but both of them lead to a wider view point of the possibilities of making money than is the case in a laborer's home. The salesman sees men who make large earnings in sales occupations which he considers practically the same as his own; and the merchant running a small business of his own looks forward to the career of a Wanamaker as his ultimate goal. Their ideas are passed along to their children.

Neither of these explanations covers fully the reasons for this apparent relationship between occupations of fathers and permanence of service on clerical jobs. Such factors are involved as the need for contributing to the support of the family, financial resources upon which to rely, and the less adequate education of children from the homes of unskilled workers. We feel certain, however, from our experience with clerical groups, that social status and brightness of prospect play a prominent rôle in the problems of turnover.

NOTES AND NEWS

The National Committee of the Eighth International Congress of Psychology, September 6 to 11, 1926, has announced that the following have consented to introduce symposia on the subjects mentioned before their names. The definite decisions of some others, who have accepted provisionally, will soon be published.

Understanding and Explanations: Binswanger, Kreuzlingen; Jaspers, Heidelberg; Spranger, Berlin.

Intensity of Sensations: Boring, Cambridge, Mass.; Myers, London; Werner, Hamburg.

Shape-qualities: Benussi, Turin; Koffka, Giessen; Michotte, Louvain.

Behaviorism: McDougall, Cambridge, Mass.; Pieron, Le Vésinet.

Psychology of the Primitive Races: Bartlett, Cambridge; Levy-Bruhl, Paris; Mayer-Gross, Heidelberg; Storch, Tübingen.

Religious Psychology: Janet, Paris; Leuba, Bryn Mawr.

Beside these the following ladies and gentlemen have expressed their intention to lecture on some special subject:

Miss Baumgarten, Solothurn; Mrs. Bühler, Vienna and Messrs. Bühler, Vienna; Buyse, Louvain; Foucault, Montpellier; Jaensch, Marburg; Julász, Budapest; Ley, Brussels; Marbe, Würzburg; Mayer-Gross, Heidelberg; Paulhan, Paris; Peters, Jena; Rignano, Milan; Rupp, Berlin; Selz, Mannheim; Spearman, London; Stern, Hamburg; Thurstone, New York; Werner, Hamburg.

Those desiring to attend the Congress are requested to send names and addresses with subscription (25 sh. per member) before January 1, 1926, to Dr. F. Roels, Maliebaan, 86, Utrecht, Holland.

At the meeting of the Taylor Society, December 2 to 5, 1925, in New York City, the following papers were offered: "The Present State of Industrial Psychology," by Lillian M. Gilbreth; "Psychology in the Organization of Prison Industries," by Edgar A. Doll; and "Purpose as a Psychological Factor in Management," by Ordway Tead. Each paper was discussed by leading psychological, prison and business experts.

The Eye Sight Conservation Council of America, Times Building, New York City, reports that defective eyesight is affecting the country's industrial output, is handicapping education and is a growing menace to human welfare. The deleterious influence of eye defects and eye

diseases is a challange to civilized effort in social control. The results of original research show that defective vision is wide-spread among industrial workers and school children and a prolific source of waste in both industry and education. Those interested in bringing about a reform in this matter are requested to communicate with this Council at the address given above.

The Winnetka Graded Book List, to be published in December by the American Library Association, Chicago, Ill., is a scientific study of children's tastes in reading which includes a list of 700 books which boys and girls have read and enjoyed, annotated with the children's own comments and based on the opinions of 36,750 children in thirty-four cities and towns located from Quincy, Mass., to Santa Fe, New Mexico. All types of schools were represented as well as a wide range of nationalities. Such a list should be of material aid to parents, librarians and teachers in selecting suitable and interesting books for each individual child.

"Basic Considerations in the Organization of Special Education for Mentally Deficient and Retarded School Children" is the title of the first report formulated by The Ohio Institute, assisted by the Central Committee on Special Education of Ohio, under the chairmanship of Miss Hannah L. Protzman, of The Ohio Institute, Columbus, Ohio.

Among the topics given consideration are the following:

1. Growing demand for the organization of special classes for mentally deficient children in the public schools.
2. Lack of coördination of special education in Ohio.
3. Organization of state committees on special education and programs of work.
4. Basic considerations to be recognized in the organization of special education for mentally deficient and retarded school children.
5. Organization of special classes in school districts having no psychological facilities.
6. Advisory service afforded through The Ohio Institute.

The Massachusetts Society for Mental Hygiene announces the first number of a new bulletin, "The Human Factor," which is expressly prepared for the busy executive in business or industry. It will appeal strongly to employers, personnel workers, educators in stores or factories, management executives, employment managers, physicians and all others who are in any way concerned with the human factor in business or industry. "The Human Factor" is under the editorial direction of Dr. Henry B. Elkind, Medical Director of the Massachusetts Society for Mental Hygiene and an unusually able consulting board. It will be sent free to members of the Massachusetts Society for Mental Hygiene and on request to all residents of Massachusetts who are interested.

BOOK REVIEWS

OSKAR PFISTER. *Love in Children and Its Aberrations.* Dodd, Mead and Company, New York, 1924. Pp. 576. Price $7.50.

This volume is translated from the German by Eden and Cedar Paul. It is the stated purpose of its author to make known to the general reader "the assured and fruitful results of the modern study of unconscious mentation, and of the unconscious in mental development." However, he assures the reader that it is no purpose of the book to supply an introduction to the practice of psychoanalysis. He insists that he hopes to lead teachers and parents to a correct understanding of the development of love in children to the end that its desirable needs may be cared for. He further believes that the awakening of love will be the solution for the ills of modern life.

In chapter one the author gives a brief survey of the history of the problem of love. He presents the consideration of thinkers from Akhenaton writing in the fourteenth century B.C. to Duboc in the twentieth century A.D. In these more than three thousand years of effort to understand love, no final and satisfactory solution is to be found. In this connection the author quotes Ibsen as saying: "Never before was a word so full of falsehood and fraud as the little word 'love' has become to-day." In the past some have found love to be an aspect of the intellect; others have found its origin in the emotions; while still a third group have made it a matter of the will. On the question of the origin and nature of love, the author gives us the following: "Love is a feeling of attraction and a sense of self-surrender, arising out of a need, and directed towards an object that offers hope of gratification."

In a brief chapter the author gives a sketch of the treatment of love in children by the great educators of history. The main body of the volume is divided into three parts each discussing a general topic. In this portion of the book we find the following questions considered: The love of the child for its parents; the love of the child for other persons than his parents; the love of the child for persons and things outside the home; the love of animals, nature, the acquirements of civilization and social institutions, the love of the child for himself and for God and divine things. There is also a consideration of the formative forces and experiences of love as well as the training of love in children and the treatment of its disorders.

The volume concludes with a fair bibliography and index. In closing, the reader is reminded that "the love sentiment and education to love

must constitute the kernel of all our educational work." It must be saved from imprisonment in the unconscious. This can in a large measure be realized through proper and adequate diagnosis and the application of the best in mental hygiene. In short, we have in this volume an endeavor to apply the method of Freudian psychology to a consideration of one of the most subtle elements of human life. How well the author succeeds depends very much upon the reader's conception of the unconscious and the possibility of making its nature and functions known.

WILLIS L. GARD,
Ohio University.

L. L. BERNARD. *Instinct—A Study in Social Psychology.* Henry Holt and Company, New York, 1924, 550 pp.

This book is a compendium of the author's findings upon the problems of instinct after fifteen years of critical and constructive inquiry. His essential thesis is that instincts do not dominate the formation of habit, as is usually supposed, and that instincts are not the chief determinants in molding the character of social institutions. Against the instinctivists Professor Bernard contends that the environmental influences, particularly the socio-psychological, are the most potent; and in this he is in full agreement with John Dewey, F. H. Allport, C. C. Josey and other recent writers upon the subject. The treatment is in the main negative, most of the space being devoted to exposing the false and conflicting uses of the term instinct. Associated with these confusing uses of the term instinct, the author argues, are the erroneous psychological theories of emotions and sentiments. Professor McDougall's position is typical of those theories brought under criticism.

The assumption upon which Professor Bernard's study rests is that "instincts and habits are defined in terms of their neural structure rather than of their value to the organism or society, and that they are to be distinguished from each other by determining their respective origins" (p. 5). The approach to the problem of the relative importance of instinct and habit for the individual and society, he holds, is through embryology, behavioristic psychology, and sociology. Instinct is defined as an inherited action pattern, as an activity process based on a neural mechanism (p. 150), as "an inborn (in the sense of inherited) process which has remained intact, that is, which has not been remade through the process of learning or of making new adjustments by means of the substitution of new stimuli or responses for the old which were inherited" (p. 84). On what is inherited he says: "Any characteristics received after the process of fertilization has taken place, that is, not contained in the chromosomes of the uniting parent cells, is not inherited. It is acquired" (p. 259).

Further illuminating portions of the study are:

1. The discussion of the manner in which such subsidiary adjustment mechanisms as reflective thought, speech and language develop as phases of habit. Language becomes externalized through writing and thus as potential habit or thought stored outside the organism, it becomes an effective environmental control process. Social institutions, also products of experience, assist in the domination of human adjustment processes, leaving instinct to play a secondary rôle. 2. The tabulation of 5684 specific instincts, a classification of these as given by standard authors, books, and cases,—all of which clearly and conclusively proves the absence of critical standards in the use of the term. 3. The analysis of what many writers call instincts of complex character, as, e.g., maternal, paternal, play, fighting, construction and acquisition. All of these and others are shown to be habit complexes with only a remote instinctive basis. 4. The critical and comparative treatment of current psychological explanations of feeling, emotion, and sentiment, showing that they are not so exclusively instinctive as supposed, but rather that the acquired elements are dominant and directive.

Students of psychology and the social sciences will feel greatly indebted to Professor Bernard for this clear and cogent argument in favor of definiteness and uniformity in the use of the term instinct. Exactness and consistency in the use of the fundamental working concepts of the social sciences is quite as important as it is in the physical sciences. The author's motive throughout the argument, namely, a concern for accuracy, truth, and utility, is certainly sound. However, it is just here that a question is likely to be raised. Few, if any, will be prepared to show that he is wrong in contending that much which psychologists and social scientists call instinct is in reality habit and therefore acquired. But it will be asked, is it correct or even most useful to interpret instinct in purely mechanistic and quantitative terms? The author charges the instinctivists with being metaphysical and mystical. May they not reply with a *tu quoque?* Only in the author's case the implication is mechanistic instead of teleological. Is it established that biology and psychology can adequately interpret their respective data exclusively in mechanistic and quantitative terms? The reviewer is in doubt about the adequacy of an exclusively mechanistic psychology as a basis for the social sciences. With this rather unnecessary implication excepted, the conclusions offered are clear gain.

MAURICE T. PRICE, *Christian Missions and Oriental Civilizations.* Published privately but obtainable from the Christian Century Press, Chicago, 1924. 578 pp.

Here in the categories of social psychology is offered to the public the first thorough scientific study of the impact of Protestant Christian

Missions upon non-Christian peoples. The point of view is unique. The missionary enterprise is regarded as a natural social phenomenon to be described in a disinterested way. The source materials gathered through twelve years of observation and investigation in America and China include: autobiographies, biographies, statements and observations by native and foreign leaders and workers—Christian, non-Christian, and anti-Christian; reports and observations of travellers and ethnologists; Western criticism of Missions and apologetics for Missions; magazines and newspapers. A vast amount of thoroughly representative data is carefully classified and interpreted as a study in "culture-contact and cross fertilization of cultures" with an emphasis upon behavior aspects rather than upon the material effects of such contacts. It is in reality a critical study of the merging of Eastern and Western civilizations. Quotations from the sources are abundant and well selected. The author seeks to make his treatment objective and has chosen for this purpose the working concepts of behavioristic and experimental psychology. Convenient summaries, interpretative in character, appear at the end of many chapters.

It is impossible in a brief review to do justice to so ambitious an undertaking, and, moreover, to one so well done. The study should be of value to leaders in the Christian Church and to any others who wish to view without prejudice the diverse reactions of individuals and groups to Christian Missions. The more minute and subtle reactions as well as the more conspicuous are interestingly depicted. Foreign trade and foreign politics are usually regarded as the chief means of culture association and culture blending. This work shows that Christian missionary endeavor is an agency of equal importance.

The author labors under no delusion as to the finality of his study. He recognizes that is is but the beginning of a type of work which for more accuracy and completeness must await the appearance of more and better materials, as well as new modes of analysis and organization. The highly technical character of the language makes of it practically a closed book to any except the trained psychologist and sociologist.

James Mickel Williams. *Our Rural Heritage*. Alfred A. Knopf, New York, 1925. 246 pp.

Our Rural Heritage is a pioneer, historical treatise on the social psychology of rural life. It is a "cross sectional description of the attitudes and beliefs" of a few typical New York communities (chiefly one) from the close of the Revolution to the end of 1874. The author believes that the historical, social psychology of a typical rural community in Iowa, Illinois, or Ohio would not be essentially different. A later book, soon to be published, will treat the rural psychology of this New York community for the remaining periods— 1875 to 1900, and 1900 to the present time.

With painstaking care Dr. Williams examined the sources, as, e.g., records of town meetings, assessment rolls, census records, school and church records, the files of newspapers, and, in addition, held hundreds of interviews with old residents. The method of treatment is thus inductive, so far as any method may be inductive. Attitudes, beliefs, and rural social processes are first observed, then interpretations are given and inference drawn. The psychological effects of the physical environment, as, e.g., climate, weather, topography, fertility of the soil, accessibility to markets and social centers, are regarded as causal in the formation of such rural attitudes as self-reliance, independence, perseverance, acquisitiveness, and resignation. Attitudes appearing in the family, attitudes to neighbors, kinship, religion, law, politics, education, and government,—all are treated critically yet with sympathetic insight and understanding. Much is made of the economic motive in the formation of these attitudes and beliefs.

The closing chapter on *Our Rural Heritage and the National Life* is significant. Here the author frankly sets up the hypothesis that the fundamental attitudes and beliefs formed in the centuries of our dominant rural life have been carried over and up into our more recent industrial and professional mode of life with little or no criticism. In this way what may have been a virtue under one mode of life becomes a vice in another and different mode of life. The author believes that before we can understand the modern agrarian movement, and, before a restoration of economic and political stability in American life can take place, serious effort must be made to understand and appreciate the basic psychological processes of rural life.

As in the case of all pioneer studies, this one will no doubt be subjected to severe criticism. However, the reviewer believes that it is all that it purposes to be,—an honest, scholarly attempt not merely to enumerate rural traits or to make a social survey, but rather to interpret psychologically and genetically rural attitudes and beliefs in respect to their causes and effects. It should be in the hands of all students of rural problems.

JOHN DEWEY. *Experience and Nature.* Open Court Publishing Company, Chicago, 1925. 443 pp. $3.00.

This book comprises ten lectures, the first series, given upon the Paul Carus Foundation. It was especially fitting that Professor Dewey should be chosen by the American Philosophical Association to deliver these lectures, since he is the leader of that school of philosophic thought most uniquely American. The author here applies his empirico-psychological method to the task of interpreting the deeper meaning of Experience, Nature, Existence, Life, Mind, Matter, Ends, Art, and Value. The point of view is genetic, behavioristic and functional.

With a positive distaste for cleavages, splits and dualisms, e.g., "between sensuous appetite and rational thought, between the particular and the universal, between the mechanical and the telic, between experience and science, between matter and mind," between means and ends, the universe with man and his achievements is viewed as of one piece, one stream, one process. Our attention is focused now upon this phase of the process, now upon that. Things *are* what they do; they are what their histories reveal them to be. Meanings and values are instrumental always, whether in science or in art. Thus the working concepts are essentially those of biology and psychology. This approach, the author believes, is concrete and scientific, the only adequate means of resolving age-long difficulties and the conflicting theories of warring schools.

WALTER S. GAMERTSFELDER,
Ohio University.

DOUGLAS FRYER. *Vocational Self-Guidance.* J. B. Lippincott Co., 1925. 385 pp.

Dr. Fryer presents this book as a result of practical experience in the vocational office. He offers it as a plan of vocational self-guidance for the intelligent man or woman. The book is not intended for the guidance of young people. The occupational information in Part II can, however, be used to advantage with the young.

The author emphasizes a fact which has often been previously stated, viz., that the individual must make his own choice of occupation. He urges the individual to investigate vocational opportunities and to study his own personality.

Dr. Harry Kitson in his introduction to the book says that "while trying to make the procedure of vocational guidance as nearly scientific as possible we must nevertheless recognize that at present it is only in a common-sense stage of development. Doctor Fryer recognizes this. He was obliged to do so while serving as a practical vocational counselor. His plans proposed in this book have, therefore, the merit of being sensible and practicable. At the same time it is his aim as a psychologist to give the work as much scientific foundation as possible. And this goal has not been lost sight of in the book. Indeed, vocational guidance is a field that requires supremely a combination of the common-sense and scientific points of view."

In spite of the author's efforts to keep the book on the best possible scientific basis, he seems to have neglected to warn the reader that the individual's judgments of his own traits and abilities are not very reliable and that many serious errors will probably accompany any plan of self-guidance. This would be particularly true of the less intelligent reader. The author should recognize more emphatically that the

judgments and assistance of others should be sought by the one who is attempting self-guidance.

Part II of the book is composed of Chapters 6 to 27 which form a series of very excellent and informing discussions of various occupations. These are contributed by persons who are experts in their fields. For the counselor and teacher this is perhaps the most usable part of the book.

Part III on "The Business Professions for Women" is very brief and written very distinctly from the point of view of the woman's right and need to engage in various types of occupational life. The author states that woman "will have many obstacles and difficulties to overcome, perhaps greater and certainly different, from those confronting the man of equal ability, but all the possibilities of success are hers and all the rewards, of money, of power, of self-expression, of service, if she but wants them, not merely enough to dream for them or to talk of them, but to *work* for them, to plan and prepare for them, and to believe both in them and in herself."

Part IV is entitled "Getting the Job." It deals helpfully with the selecting of the job and sales methods.

The book as a whole contains material which can be used particularly in the guidance of adults. It could be put into the hands of the adult himself, although as the author says, it would have to be an intelligent adult who could use it to advantage. His best use of it, however, would be in coöperation with the expert counselor or psychologist.

THOMAS C. MCCRACKEN,
Ohio University.

L. L. THURSTONE. *The Fundamentals of Statistics.* New York, The Macmillan Company, 1925. 237 pp.

ARTHUR S. OTIS. *Statistical Method in Educational Measurement.* Yonkers-on-Hudson, New York, World Book Company, 1925. 337 pp.

HAROLD RUGG. *A Primer of Graphics and Statistics for Teachers.* Boston, Houghton Mifflin Company, 1925. 142 pp.

Early books on statistical method were written by scholarly scientists, usually well-trained in mathematics, and were read and understood by a limited number of individuals whose interest in statistics and whose knowledge of mathematics permitted them to follow the development of the subject. With the extension of statistical methods from the original field of biology to the fields of economics, psychology, sociology, and educations, however, it soon became apparent that statistical treatises, presupposing no more mathematical training than is given in the secondary school, must be prepared. That the efforts of Thorndike, the Eldertons, King, Rugg, Secrist, Jones, and others, to meet this need

were only partially successful is witnessed by the large number of statistical books published in the last two or three years. In addition to the three under review, we have Odell, Mills, Kent, Riegel, Forsyth, Jerome, and others. The outstanding scholarly work of the present day—Kelly—and the technical handbook by Rietz, *et al.* are standard reference works for the more advanced student.

Of the three books under consideration, Otis and Rugg were written primarily for the profession of education while Thurstone is a more general treatment, although the editor—O'Shea—in his introduction assumes that the book will be read chiefly by teachers. Otis and Thurstone are considerably more elaborate discussions than is Rugg, probably occupying, so far as reading difficulty is concerned, a position about midway between Rugg's two books—his *Statistical Methods Applied to Education* and his *Primer*. Each of the three books naturally possesses some unique features.

Rugg is brief, concise, and reasonably clear. The usual measures of central tendency and variability are briefly described and well illustrated. Simple correlation is computed by three methods. The book is profusely illustrated; it contains 115 figures in its 142 pages. (Thurstone has 40 figures in 237 pages and Otis, 68 figures in 337 pages.) For teachers' meetings and short course normal school classes, this book will be popular and justly so.

Thurstone is brief and non-technical but makes much better provision for an understanding of fundamental mathematical concepts than is usual. The book has chapters on Linear Relations, Non-linear Relations, The Equation of a Straight Line through the Origin, The General Equation of a Straight Line, The Binomial Expansion, and The Probability Curve, as well as the usual discussion of the tabulation of data, graphic representation, and measures of central tendency and position, variability, and relationship. The discussions are adequate for an elementary course but clear and comprehensible. Unfortunately, the book is not supplied with references or a bibliography and makes no mention of partial or multiple correlation.

Otis has prepared the most elaborate book of the three. Explanations are more detailed in character, and probably more lucid to some beginners, than is the case in either Rugg or Thurstone. In the reviewer's judgment, the discussion and interpretation of correlation is not only the best feature of the book but the best available. Interpretation is accomplished by numerous examples and illustrations and by introducing Kelly's *coefficient of alienation*, a concept of great importance and value. Partial and multiple correlation are briefly treated. In the opinion of the reviewer, the book is overloaded with discussions of percentile curves and graphs and is faulty in that illustrative test material comes largely from the publications of one company.

With the appearance of these three books it should no longer be necessary for educators to be unacquainted with elementary statistical analyses and interpretations.

R. L. MORTON,
Ohio University.

STANLEY HIGH. *Europe Turns the Corner.* New York, The Abingdon Press, 1925. Pp. 308. $2.00.

The author bases his work on observation and first hand contacts with the changing and improving Europe. As a student of political and economic conditions during several trips to Europe in the last five years, he concludes that the method and spirit of war have given place to peaceful overtures and that the program of revenge has been superceded by a policy of co-operation. Although the issues have not been settled, they have ceased to be threats of war.

The British labor government under Ramsay MacDonald was the beginning of the "turn;" the program of Mussolini in Italy, although unfortunate in its international aspects, has contributed through its worthy domestic features; the French have abandoned their policy of revenge and their ambition to destroy Germany; and the new Russia is emerging through education, religion, and intercourse with other peoples. The author sees in the Baltic League, the Little Entente, the financial and economic co-operation with Hungary, and other movements the development of attitudes which may make possible a United States of Central Europe.

Without the United States, Europe proceeds in the League. Indications of a transformed Europe are noted in the labor government in England, the downfall of Poincare in France, the London Conference, the Dawes plan, the recognition of Russia by England and France, and the pronouncement of the Protocol of the League. Besides an account of the development of these factors, the author presents an analysis of them and their implications in international affairs. The significance of these interests for the security and prosperity of the United States is stressed.

Events of 1925, such as the Locarno Conference with its hopeful prospects, confirm the author's optimistic position that European conditions offer the bases for a new structure of world society. It remains to be observed whether the concept of the world as a social group can be retained for a period long enough to enable such attitudes to become habitually established. Colonel Edward M. House, who has written the introduction, concludes: "He has written his story well and understandingly, and coming generations who would know of these times will be his grateful debtors."

Psychology and History. HARRY ELMER BARNES, Professor of Sociology in Smith College. The Century Co., New York. 1925. Pp. 195.

The author brings to this study the rich experience of the historian and the sociologist who has been influenced also by stimulating contact with the late President G. Stanley Hall of Clark University. The dedication of his more extensive work "The New History and the Social Studies," of which this book is the reprint of two chapters, indicates the author's debt to the "Columbia School of Historians" which was led by James Harvey Robinson. The book presents "a serviceable summary of the major developments in social psychology which are significant for the historian," and for the psychologist "a review of the efforts which have been made by historians . . . . to apply psychological methods and data to the interpretation of the history of man and human culture." The author gives sarcastic evidence of irritation with "respectable" historians' viewpoint that their only service is to make the past known without concern whether that past has any serviceable connection with the present. He believes that the interpretation of history is dependent upon an understanding of the principles of individual and of social psychology. Although the former is valuable in interpreting the personalities of historic periods, the latter is necessary to understand the development of the social group. The author concludes that "at the same time that social psychology was being elaborated there was developing a genetic view of history, the idea of continuity of history, which is based upon the knowledge that our present cultural complex is primarily the product of a long inheritance from past conditions and that the present can be understood only when viewed in the light of its historical antecedents. When this attitude was accepted by the more progressive students . . . . social psychology had put at their disposal the methodology and point of attack which was necessary to show how the modern age had come about . . . . if the collective psychology was an all-important factor in modern life it should have had a great, if not equal, significance in all ages. The systematic following up of this clue has constituted essentially the psychological interpretation of history in so far as it has been developed." The reader is introduced to the psychology of instincts, behaviorism, and the psychology of the unconscious as the more pronounced innovations for the consideration of those concerned with the psychological interpretation of history.

After a brief "Prefatory Note" the author introduces, with generous quotation, the conspicuous contributors to the fields of psychology and history who have discussed the more important aspects of the development of psychology in relation to history and the social studies. The author has arranged his materials to demonstrate that the psychologist has made contributions which may be utilized by the historian to produce a utilitarian type of history. The illustrative materials, calculated

to demonstrate the principles presented, admirably suggest the values of the psychological approach. To the psychologist, this study presents a summary of the work of those who have tried to utilize psychological contributions in producing historical writing; and to the historian, it lends encouragement to apply the principles of psychology to the interpretation of the past for the understanding of the present.

EDWIN B. SMITH,
Ohio University.

NEW BOOKS AND PAMPHLETS RECEIVED

Books and pamphlets for review should be sent to James P. Porter, Department of Psychology, Ohio University, Athens, Ohio.

L'Avenir du Travail. Bulletin de l'Association Internationale pour la Protection légale des Travailleurs. Vol. III, Nos. 1-2. 1925. Imprimerie, Librarie Berger-Levrault, Paris. 75 pp.

Contributions to Education, Teachers College, Columbia University:

A Comparison of the Intellectual and Educational Status of Neurotic and Normal Children in Public Schools. Esther Katz Rosen. No. 188.

Comprehension Difficulties of Ninth Grade Students in the Study of Literature. Theo. W. H. Irion. No. 189, 1925.

An Experimental Method for the Discovery and Development of Tests of Character. Theodore P. Lentz, Jr. No. 180, 1925.

The Measurement of Fair-Mindedness. Goodwin B. Watson. No. 176, 1925.

The Opposites Test. Andrew Tennant Wylie. No. 170, 1925.

A Study of the Relationship Between Rate and Ability. Lillian May Hunsicker. No. 185, 1925.

The Constructive Ability of Young Children. Lovisa C. Wagoner. University of Iowa Studies, First Series No. 94, June, 1925. Iowa City. 55 pp.

The Emotions of Young Children. Leslie R. Marston. University of Iowa Studies, First Series No. 95, June 15, 1925. Iowa City, Iowa. 99 pp.

Europe Turns the Corner. Stanley High. The Abingdon Press, New York. Price $2.00. 308 pp.

Health and Safety in the New Curriculum. Geo. E. Payne and Louis C. Schroeder. The American Viewpoint Society, Inc., New York, 1925.

An Introduction to Objective Psychology. G. V. Hamilton. The C. V. Mosby Company, St. Louis, Mo., 1925. Price $5.00.

Mental Measurement in Educational and Vocational Guidance. Harvard Bulletins in Education. Cambridge, Mass. No. X, November, 1924.

Modern Theories of the Unconscious. W. L. Northridge. E. P. Dutton & Co., New York City. Price $3.75. 192 pp.

Occupational Efficiency of the Mentally Defective. Bulletin of the University of Minnesota, College of Education. Vol. 27, No. 55, December 20, 1925.

Preliminary Report on Supervision in County Demonstrations. Educational Bulletin No. 74. Dept. of Public Instruction, State of Indiana, 1924.

A Primer of Psychology. EDWARD BRADFORD TITCHENER. The Macmillan Co., New York. 1925.

Psychology and History. HARRY ELMER BARNES. The Century Co. New York. Price $1.50. 195 pp.

The Recent Foreign Policy of the United States. GEORGE H. BLAKESLEE. The Abingdon Press, New York. Price $2.00. 368 pp.

Religious Education Survey Schedules. WALTER S. ATHEARN. Vol. III. George H. Doran Co., 1924.

Reorganization and Administration of an Elementary School to Meet the Needs of a Community. University of California Publications in Education. Vol. 2, No. 2, Berkeley, Calif., 1925.

Report on the Present Position of Vocational Guidance and Vocational Selection. M. BOOLE STOTT. The Women's Employment Publishing Co. Ltd., London, England.

Report of the Survey of the Public Schools of Philadelphia. The Pennsylvania State Department of Public Instruction, Four Books, 1922.

Special Disabilities in Learning to Read and Write. ELIZABETH E. LORD, LEONARD CARMICHAEL AND WALTER F. DEARBORN. Harvard Monographs in Education, Series I, Vol. II, No. 1. The Graduate School of Education, Harvard University, Cambridge, Mass. Price $1.00. 76 pp.

Studies of Mental Defects and Handicaps. J. E. WALLACE WALLIN. Miami University Bulle in, Series XXII, No. 5. Oxford, Ohio. Price 75 cents. 177 pp.

A Study of Supervised Study. WILLIAM ARTHUR BROWNELL. University of Illinois Bulletin, Vol. XXII, No. 41. Urbana, Ill. Price 50 cents. 48 pp.

The Taxation Amendment. J. W. TANNEHILL. Superintendent of the Building and Loan Department, Room 409, State Office Building, Columbus, Ohio. 10 pp.

Tryout Set, McQuarrie Test for Mechanical Ability. Associated Students Store, University of Southern California, Los Angeles, Calif.

The Visiting Teacher in Rochester Report of a Study. MABEL BROWN ELLIS. Joint Committee on Methods of Preventing Delinquency, 1925.

Department of the Interior Publications, Department of the Interior, Washington, D. C.:

Accredited Secondary Schools in the United States. FRANK M. PHILLIPS. Bulletin No. 11, 1925. Price 15 cents. 119 pp.

Helps for the Rural School Nurse. HARRIET AND HAZEL WEDGWOOD. Health Education Bulletin No. 17, 1924.

Introduction of Algebra into American Schools in the Eighteenth Century. LAO GENEVRA SIMONS. Bureau of Education Bulletin, No. 18, 1924.

Land-Grant College Education, 1910–1920. Part III. WALTON C. JOHN. Bulletin No. 4, 1925. Price 25 cents 108 pp.

List of Publications Available September, 1925. 24 pp.

List of References on Higher Education. Bureau of Education, Library Leaflet, No. 28, December, 1924.

Professional Staff of State Departments of Education. ARTHUR WESLEY FERGUSON. Bulletin No. 17, 1925. Price 10 cents. 64 pp.

The School as the People's Clubhouse. HAROLD O. BERG. Physical Education Series No. 6, 1925.

Statistics of Kindergartens, 1923–24. FRANK M. PHILLIPS. Bulletin No. 20, 1925. Price 5 cents. 7 pp.

Statistics of State Universities and State Colleges, for Year Ending June 30, 1924. Bulletin No. 12, 1925. Price 5 cents. 23 pp.

Teacher and Pupils' Reading Circles. ELLEN C. LOMBARD. Home Education Circular No. 7, March, 1925.

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